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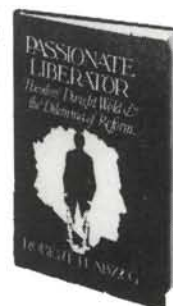
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American Historians on the European Past

DAVID H. PINKNEY

SIR HANS SLOANE, THAT TIRELESS SEARCHER AFTER COLLECTIBLES whose vast accumulations became the nucleus of the British Museum, wrote on the flyleaf of his *Natural History of Jamaica* a verse from the Book of Daniel that is an appropriate preface to this essay. "Many shall run to and fro," it read, "and knowledge shall be increased." Many American historians of Europe have run to and fro across the Atlantic in recent decades, and historical knowledge has indeed been increased. I am less certain that its quality matches its quantity.

I made my first trip across the Atlantic in 1936, not yet as a historian but as a recent college graduate attracted to the study of modern European history by my college teacher, Frederick Artz. In Europe I was fascinated by the living presence of history that I had, until then, seen only in the reflection of other men's words. The rich evidence of Britain's and the Continent's past impressed on me more than any books the long course of European history, the immense complexity of its many interweaving national elements, and the strangeness of much of it to an American raised and educated in the Middle West.

I recall asking myself in the autumn of 1936, when I entered graduate school at Harvard, if I should not turn from European history, which then loomed so formidably, to the history of the United States, to which I could bring the understanding of a native son. I did not make the change, and I have no regrets, for I have found European history endlessly fascinating. Yet the question has remained with me, not in a personal way but as a question for all Americans of my branch of the profession. Can Americans, making their careers in the United States or Canada,¹ be significantly productive historians of modern Europe or of any of its national states?

This presidential address was delivered at the Ninety-Fifth Annual Meeting of the American Historical Association, held in Washington, D.C., December 28, 1980. I am much indebted to my two research assistants, Margaret Merrill and Alden Jencks, who provided me not only with succinct summaries of scores of book reviews but also with many perceptive insights into Soviet and German reception of the works of American historians. A number of friends and colleagues generously helped guide me through fields of history with which I am little familiar or saved me from errors in my own field. I am especially grateful to Charles Delzell, Fred Levy, Charles Mullett, Stanley Payne, Fritz Stern, Peter Sugar, Edward Tannenbaum, Donald Treadgold, Henry Ashby Turner, Joan Ullman, Wayne Vucinich, Daniel Waugh, and Gordon Wright.

¹ For this essay I have defined "American historian" as a historian who is a citizen or long-term resident of the United States or Canada who had his or her training in an American or Canadian university or, in the case of a scholar working in Continental European history, in a British university.

My colleagues in French history know that this is essentially the question to which I addressed two articles on what I called "The Dilemma of the American Historian of Modern France."² The dilemma is the choice between writing monographs based on exhaustive research in archives in the manner of the French university historians of our time or writing books that are primarily syntheses of monographic scholarship, intended for American and English readers, whose contribution for the international scholarly community is an outsider's insight into the history of France.

The first year of the 1980s is an appropriate time to consider the accomplishments and failures of American historians of all of modern Europe. Just half a century has passed since the establishment of the *Journal of Modern History*, a landmark in the development of modern European history as a professional academic field in America. At that time—the late 1920s—the number of American scholars in the field was small. Chester P. Higby of the University of Wisconsin, the principal moving spirit behind the effort to create a journal of modern European history, identified 250 American historians of modern Europe in 1926, when he circulated a questionnaire on teaching, research, and writing in the area. The number of replies to that questionnaire—160—is itself eloquent testimony to the smallness of this group of historians just over fifty years ago. Their replies reveal a narrow range of scholarly interests. The principal national concentrations were in British history and French history, and the most popular research topics were World War I and the French Revolution. Although more than one-fifth of those who replied had received at least some of their training in Germany, few worked in German history, and even fewer in Eastern European history. Fewer than one-half had ever done research in European archives and libraries.³

THE MOST WIDELY ACCLAIMED AMERICAN CONTRIBUTION to modern European historical scholarship in the interwar years was in diplomatic history, and two of its landmarks—Sidney Bradshaw Fay's *The Origins of the World War* (1928) and Bernadotte Schmitt's *The Coming of the War, 1914* (1930)—appeared shortly after Higby made his inquiry. The American concern with European problems that those books reflect contributed in succeeding decades toward turning more American scholarly effort to the study of modern European history. Events of the 1930s—the Great Depression, the fall of the German republic, the spread of fascism, and the menace of Nazi Germany—gave a new immediacy to Euro-

² Pinkney, "The Dilemma of the American Historian of Modern France," *French Historical Studies*, 1 (1958): 11–25, and "The Dilemma of the American Historian of Modern France Reconsidered," *ibid.*, 9 (1975): 170–81.

³ Higby, "The Present Status of Modern European History in the United States," *Journal of Modern History*, 1 (1929): 6–7; William H. McNeill, "A Birthday Note," *ibid.*, 51 (1979): 1–2; Henry Cord Meyer, *Five Images of Germany: Half a Century of American Views of German History*, American Historical Association Publication, Service Center for Teachers of History, no. 27 (2d ed., Washington, 1960), 13; and John S. Curtiss, "History," in Harold H. Fisher, ed., *American Research on Russia* (Bloomington, Ind., 1959), 24–25.

pean developments and a pressing urgency to understanding them. A new breed of European historians, specialists in the internal histories of individual European nations, began to emerge in growing numbers and to move onto ground formerly occupied by native European historians. They produced largely two types of books: (1) syntheses of European scholarship supplemented by findings of their own limited research in primary sources (such as Crane Brinton's *A Decade of Revolution, 1789–1799* [1934] and Frederick B. Artz's *France under the Bourbon Restoration, 1814–1830* [1931]), an American genre to which Leonard Krieger gave the inelegant but descriptive label “the refinished import”;⁴ and (2) original, archive-based monographs on the European model. As the decade of the 1930s moved into the 1940s, the appearance of sophisticated monographs (such as *The English Yeoman under Elizabeth and the Early Stuarts* [1942] by Mildred Campbell, *Electoral Procedure under Louis-Philippe* [1937] by Sherman Kent, *Nationalism and the Cultural Crisis in Prussia, 1806–1815* [1939] by Eugene Newton Anderson, and *Church and State in Russia: The Last Years of the Empire, 1900–1917* [1940] by John S. Curtiss) testified to the growing interests and competence of American historians of Europe and to the capacity of American graduate schools to train students in this kind of historical scholarship.

Nevertheless, although the producing scholars in modern European history then included some distinguished practitioners, their number remained small. My master, the late Donald McKay, remarked that the historians of France in the United States in the 1930s could hold their annual meeting on a sofa. The “take-off” of modern European history as a populous and prolific field of scholarly activity in the United States and Canada came after the World War of 1939–45. That activity was fueled by a variety of potent forces. The war intensified interest in Europe and in a search to discover what had produced the catastrophes of war, economic collapse, and social upheaval. A generation of historians fresh from graduate school had the heady experience of participating in rapidly moving history, many of them on military staffs or in intelligence organizations where their historical knowledge and skills gave them opportunities of privileged observation. Beginning in the late 1940s the Cold War added to concern with Europe and to the desire for understanding. The study of history promised answers to pressing questions. The “GI Bill” enabled hundreds to attend graduate school to pursue their interest in history. Foreign languages learned in army or navy schools facilitated specialization in hitherto unusual fields. The extraordinary strength of the American dollar in the quarter-century after the war made extended periods of research in Europe practical for professors and graduate students as never before—or since. After 1949 Fulbright fellowships provided the means for hundreds of young Americans to study and to do research in Europe, and in 1958 the introduction of jet airplane service on the North Atlantic route brought European archives and libraries within a few hours’ travel time of major American cities. Perhaps even more significant was

⁴ Krieger, “European History in America,” chapter 4 of John Higham with Krieger and Felix Gilbert, *History* (Englewood Cliffs, N.J., 1965), 275–76.

the influx into America in the 1930s of refugee scholars, especially from Germany and Austria—Hans Baron, Andreas Dorpalen, Dietrich Gerhard, Felix Gilbert, Hajo Holborn, Hans Kohn, Theodor Mommsen, Franz Neumann, and others; they enriched the quality and extended the range of graduate study in European history and attracted able young Americans into the field. Then, too, many younger Europeans who fled Europe in the 1930s or 1940s as children or adolescents took up the study of European history, motivated in part at least, one may assume, by the desire to learn more of the forces that had convulsed their world and disrupted their lives. They usually brought to the task language skills still rare in America and the advantage of being able to view European history as both outsiders and insiders. The influx of talent of both foreign mature scholars and foreign students was in its intellectual influence probably unparalleled in modern experience. European history in America was one of the great beneficiaries.⁵

The upsurge of university and college enrollments in the postwar decades created a market for historians, and European history became an economically viable occupation for all, losing its aura of a gentleman's avocation, which it still retained even in the 1930s. In the postwar decades jobs were waiting. Established graduate schools expanded, new schools emerged, and the narrow stream of Ph.D.'s in history flowing from graduate schools widened into a broad river.

These new Ph.D.'s were professionally formed in a milieu that emphasized research—usually archival research—and publication, and in the 1950s their manuscripts appeared in growing numbers on publishers' desks. Established and newly created university presses, buoyed by foundation grants and public support, were able to publish hundreds of them. New, specialized journals (such as the *Journal of British Studies*, *French Historical Studies*, the *Journal of Central European Affairs*, and the *Austrian History Yearbook*) permitted the publication of a surging torrent of articles. The little band of European historians who in 1929 founded the *Journal of Modern History* and the Modern European History Section of the American Historical Association had hoped, they said, by their action "to promote the study of European history in North America." That hope has, in the words of William H. McNeill, writing in the fiftieth anniversary number of the journal, "been accomplished on a scale they hardly dreamed of." In the years from 1968 to 1978 alone, according to a count made by McNeill, American historians published more than 2,000 books on European history since 1750.⁶

The quantitative accomplishment of our modern European historians is clear and undoubted. The focus of my concern is on the quality and significance of their achievement in the thirty-five years since the beginning of the great "take-off" of the mid-1940s. Admittedly, no method of measuring quality in historical writing is definitive, and any method that one may adopt has limitations that

⁵ See H. Stuart Hughes, *The Sea Change: The Migration of Social Thought, 1930–1965* (New York, 1975), 1–2.

⁶ McNeill, "Birthday Note," 3, and "Modern European History," in Michael Kammen, ed., *The Past before Us: Contemporary Historical Writing in the United States* (Ithaca, N.Y., 1980), 96–97.

another investigator might reasonably find unacceptable. I have chosen to use the formal judgments of American books made by professional historians in the European countries about whose histories we have written. I have looked to the English historical establishment for a measure of the quality of American books on English history, to the French university historians for judgments of American books on French history, and, similarly, to West German and Soviet historians for judgments of our works on their histories. I have read the reviews of American books on their respective histories in three leading journals in Britain, three in France, and three in Germany and in two Soviet journals in the thirty-five years between 1945 and 1980 or for shorter periods in cases in which the journals have been founded since 1945. I have classified the judgments the reviewers expressed according to five categories: Enthusiastic, Favorable, Neutral, Reserved, and Unfavorable. "Enthusiastic" I have restricted to those that acclaim books as definitive and likely long to remain so or as path-breaking renewals of their subjects. "Neutral" designates those reviews that describe or summarize a book without offering any clear judgment of it. And "Reserved" denotes those reviews that express serious reservations about a book yet do not make an overall unfavorable judgment.

GEOFFREY ELTON, WRITING IN 1970 in his *Modern Historians of British History*, declared that the preceding quarter of a century had wrought a "remarkable transformation in our understanding of English history since the accession of the Tudors." There had been, he declared, "a major renewal" and, in some areas, "a total reconstitution." He attributed this to the substantial increase in the number of historians working in the field, to greater professional competence, to the multiplying of accessible sources, and to the concern with new questions and the use of new methods of research and analysis.⁷ My question here is, What has been the contribution of American historians of Britain to this transformation and to its continuation in the decade since Elton made this judgment?

In the thirty-five years beginning in 1945 the *English Historical Review* reviewed 289 books in British history written by Americans, 242 of which were reviewed in "Short Notices," which sometimes run to as much as a page and a half, and 47 in longer "Reviews." *History*, the journal of the Historical Association, reviewed 221, and the *Historical Journal* and its predecessor, the *Cambridge Historical Journal*, reviewed 64. No other foreign journal that I surveyed reviewed so many American books in these three and a half decades as did the *English Historical Review* and *History*; only the French in the *Revue historique* approached these figures, with almost two hundred reviews during the same period. The annual coverage in the *English Historical Review* rose from three to four reviews in the 1940s to an average of nine during the 1950s, ten during the 1960s, and thir-

⁷ Elton, *Modern Historians of British History, 1485-1945: A Critical Bibliography, 1945-1969* (Ithaca, N.Y., 1970), 1.

TABLE 1
Modern British History: American Books Reviewed, 1945-79

Judgment	<i>English Historical Review</i>	<i>History</i>	<i>Cambridge Historical/ Historical Journal</i>
ENTHUSIASTIC	4	10	0
FAVORABLE	170	130	32
NEUTRAL	4	10	3
RESERVED	71	36	24
UNFAVORABLE	40	35	5
TOTALS	289	221	64

teen during the 1970s. *History* follows a similar progression, usually noting somewhat fewer books, but in 1978 alone its editors found 25 books worthy of review.

The generally favorable judgment (see Table 1) of the products of American scholarship is impressive. Of the 289 American books reviewed in the *English Historical Review*, 174—fully 60 percent—were favorably received and only 40—14 percent—unfavorably. The incidence of both “Favorable” and “Unfavorable” notices in *History* was slightly higher. In the *Historical Journal* the proportion of favorable reviews dropped to 50 percent, and “Reserved” reviews were more frequent than in the other journals, which is probably not surprising to its regular readers. The *Historical Journal*’s reviewers were enthusiastic about none of the books they considered, but *History* gave that accolade to 10 books and the *English Historical Review* to 4. In almost none of the reviews did I find any hint of condescension or disdain for colonials or outsiders. The English historical establishment, this record indicates, accepts American historical scholars as equals on its own ground, writing the same kinds of scholarly books as do the English themselves, books intended for scholarly readers on both sides of the Atlantic.

The occasional enthusiastic review and other comments suggest, moreover, that English scholars regard some American historians as their masters in the study of their own history. Reviewing the *Festschrift* for Wallace Notestein, Christopher Hill of Balliol declared, “Notestein is among the great seventeenth-century historians. His *Winning of the Initiative by the House of Commons* is a classic, fundamental to all our thinking about the constitutional conflicts in Stuart England.” D. H. Pennington of the University of Manchester on the same occasion avowed that Notestein was “firmly enthroned, with Neale and Namier, as one of the ‘three N’s’ of English parliamentary history.”⁸ Recently, Notestein’s conclusions have been challenged, but this neither erases the high praise of the 1960s nor diminishes my argument. Notestein was already an important and recognized scholar before World War II (his *Winning of the Initiative by the House of*

⁸ Hill, Review of William Appleton Aiken and Basil Duke Henning, eds., *Conflict in Stuart England: Essays in Honour of Wallace Notestein* (London, 1960), in the *English Historical Review* [hereafter, *EHR*], 76 (1961): 681; and Pennington, Review of Aiken and Henning, *Conflict in Stuart England*, in *History*, 46 (1961): 57.

Commons was published in 1925), but younger men have also been recognized for their major contributions to the renewal and transformation of modern English history that Elton noted. In the mid-1950s Christopher Hill wrote in the *English Historical Review*, "The best writing in English on our seventeenth century is today coming out of the United States," and in 1967 in his review of *The World We Have Lost* he rebuked Peter Laslett for being "ignorant of the important sociological history written in England and the USA during the past twenty-five years."⁹ A critic writing in the *Historical Journal* in 1975 commented on J. H. Hexter's *The Reign of King Pym* (1941), noting that "no book has exerted a more profound influence on the history of the revolutionary middle decades of the seventeenth century." And a reviewer of Wallace J. MacCaffrey's *The Shaping of the Elizabethan Regime* declared, "This is narrative history at its best."¹⁰ At the highest level of achievement and recognition—along with Wallace Notestein—stands Arthur Marder. His "magisterial studies on British sea power," declared a reviewer in the *English Historical Review* in 1976, are "an oeuvre that is already one of the ornaments of mid-twentieth-century scholarship."¹¹ He is recognized as the historian of the Royal Navy in the twentieth century. Oxford University's granting him an honorary doctorate of laws and the crown's bestowal upon him of the Order of the British Empire dramatically acclaim the high and secure place of his contribution to English historical literature. No American working in modern French history has ever won such recognition from his or her French peers.

An explanation of this contrast can be found in the different standards of judgment applied to American books on opposite sides of the Channel. Among university historians in France the measure of a great historical work is the *thèse* of the *doctorat d'état*, a massive product of a decade or more of research aimed at total coverage of all relevant archives and other sources. Americans living three thousand miles or more away and bound there by their employment cannot spend the years in French archives necessary for the preparation of works comparable to the French *thèse*. England has no such monumental academic model. Even the most distinguished English historians rarely produce works of the sheer size and majesty of Georges Lefebvre's *Les Paysans du Nord pendant la Révolution française* (1924; 2d ed., 1959) or André-Jean Tudesq's *Les Grands notables en France (1840–1849): Étude historique d'une psychologie sociale* (1964). English dissertations are ordinarily completed in three or four years and are relatively brief. A year and one or two summers of research in British archives and libraries usually suffice to match this model, and they are not difficult to arrange. Clearly, many

⁹ Hill, Review of William Haller, *Liberty and Reformation in the Puritan Revolution* (London, 1955), in the *EHR*, 71 (1956): 286, and Review of Laslett, *The World We Have Lost* (London, 1965), in *History and Theory*, 6 (1967): 126 (italics added).

¹⁰ Margaret Ashton, Review of John R. MacCormack, *Revolutionary Politics in the Long Parliament* (Cambridge, Mass., 1973), and of Blair Worden, *The Rump Parliament, 1648–1653* (Cambridge, 1974), in the *Historical Journal*, 18 (1975): 178; and F. D. Price, Review of MacCaffrey, *The Shaping of the Elizabethan Regime* (Princeton, 1968), in the *EHR*, 87 (1972): 185.

¹¹ Edmund Ions, Review of Richard D. Challener, *Admirals, Generals, and American Foreign Policy, 1898–1914* (Princeton, 1973), in the *EHR*, 91 (1976): 454.

Americans have done so and have equaled and even excelled the British on their own ground. Historians in North America have produced monographic studies of the highest quality and occasionally of superlative importance. If we accept the collective judgment of the British historical establishment, we can conclude that our historians of Britain have been fruitfully engaged in a proper course of historical research and publication.

DESPITE THE FORMIDABLE FRENCH STANDARD raised before them, scores of Americans do write French history. The number of their books on the modern period published in the past three and a half decades is probably second only to those on modern British history. Of the 2,000 books that McNeill found in his survey of American works on European history since 1750, 304 were on France, compared with 543 on Britain.¹² The *Revue historique* published reviews or notices of 194 American books between 1945 and 1979. *Annales: Économies, sociétés, civilisations* and the *Revue d'histoire moderne et contemporaine* together reviewed 62.

The American interest in French history predates World War I, but the tumultuous course of events in the 1940s—the astounding collapse in 1940, the puzzling episode of Vichy, the Resistance, the rise of de Gaulle, the Liberation—heightened interest in France, and the turmoil of postwar adjustment combined with France's key position as the nearest American bridgehead on the Continent sustained that interest at a high level in the postwar years. If it declined in the 1950s, the return of de Gaulle to power revived it at the close of the decade. French history appeared as an important and lively subject to many young Americans, especially to those whose wartime duties had taken them to France or involved them in the study of France in military or political intelligence agencies. In 1956 the growing number of American historians of France made possible the establishment of the Society for French Historical Studies, which henceforward held annual conferences on French history and two years later created a journal, *French Historical Studies*, the first outside France, I believe, devoted exclusively to the publication of scholarly articles on French history.

The scholarly production of this considerable body of American historians has been noted with increasing favor in the leading French historical journals (see Table 2); and the proportion of favorable reviews has risen from just under 40 percent in the 1950s to just over 60 percent in the 1970s. The once-common expressions of condescension toward American scholarly efforts, moreover, have almost disappeared. But the number of books reviewed in the 1970s, when more were appearing, was no larger than in the 1950s, and few American books have been judged worthy of translation and publication in French (in contrast with the practices of other Western European countries), nor have any American historians of France won recognition from their French peers comparable to that won by American historians of Britain from their British judges. No American

¹² McNeill, "Modern European History," 97.

TABLE 2
Modern French History: American Books Reviewed, 1945-79

Judgment	<i>Revue historique</i>	<i>Annales: Économies, sociétés, civilisations</i>	<i>Revue d'histoire moderne et contemporaine, 1954-</i>
ENTHUSIASTIC	6	3	0
FAVORABLE	99	31	7
NEUTRAL	27	4	1
RESERVED	7	9	5
UNFAVORABLE	55	2	0
TOTALS	194	49	13

has written a book that the French rank with Lefebvre's *Les Paysans du Nord*, Emmanuel LeRoy Ladurie's *Les Paysans du Languedoc* (1966), or, of course, Fernand Braudel's *La Méditerranée et le monde méditerranéen à l'époque de Philippe II* (1949; 2d ed., 1966). Some French scholars closely follow American work in their fields, but one still encounters in professional writing dismaying ignorance of—or indifference to—American publications. For example, a historiographical article on the Second Empire, published in 1974, mentions only one book by an American,¹³ although American scholars have contributed significantly to the renewal of the history of that regime. The basic explanation of the French establishment's limited approval of American historical scholarship is to be found, I think, as I suggested earlier, in the French model of what constitutes a great work of history. The French standard, the *thèse*, can never be met by Americans—or by anyone else so far removed from the archival sources of French history.

Yet there are fields in modern French history in which the work of American historians is winning growing recognition as important scholarship, even superior to that of the French themselves. This has occurred usually in areas that French historians have neglected. There foreigners have been able to move onto unplowed and sometimes fertile ground. Thirty-five years ago the eminent French colonial historian Charles A. Julien called attention to American work in colonial history and warned his French readers, "At present it is impossible seriously to study French colonization without knowing English."¹⁴ On the reign of Louis XIV, Jean Meyer of the University of Paris IV recently declared in the *Revue historique* that "the young and dynamic school of French history" in America has since 1945 accomplished "a profound renewal of diplomatic history" and an almost complete revision of the generally accepted view of the closing years of Louis's reign. This work, little noticed by the French historical school, must, Meyer insisted, eventually be incorporated into "our historical vision." In his very complimentary review of A. Lloyd Moote's *The Revolt of the Judges*, Meyer

¹³ Pierre Guiral and Émile Témime, "L'Historiographie du Second Empire," *Revue d'histoire moderne et contemporaine*, 21 (1974): 2-17.

¹⁴ Julien, *Les Techniciens de la colonisation (XIX^e-XX^e siècles)* (Paris, 1947), vi.

observed that the reflections of an author detached from the French historical school can be salutary and that intimate knowledge of other national histories enables Anglo-Saxon historians to make illuminating comparisons.¹⁵

Another area where American achievement is recognized and praised by French historians is twentieth-century history, especially of the years between 1918 and 1945. In their own country French historians have, in the words of John C. Cairns, "to an unusual degree surrendered contemporary history to journalists and politicians."¹⁶ American historians moved into the field, and they brought to their study of it the advantages of perspective and detachment. Americans, like Englishmen, have never experienced a foreign invasion or a mis-carried revolution, and for them the study of contemporary history, one French historian recently observed, creates "no state of anguish."¹⁷ Such books as Joel Colton's *Léon Blum: Humanist in Politics* (1966), Philip C. F. Bankwitz's *Maxime Weygand and Civil-Military Relations in Modern France* (1967), Eugen Weber's *Action française: Royalism and Reaction in Twentieth-Century France* (1962), and Robert O. Paxton's *Parades and Politics at Vichy: The French Officer Corps under Marshal Pétain* (1966) earned the admission of René Rémond of the University of Paris X in 1970 that "to Americans we owe some of the best studies of contemporary France."¹⁸ Paxton's *Vichy France: Old Guard and New Order, 1940-1944* (1972), published after Rémond made this statement, has won comparable recognition.

In the past decade, however, young French historians, apparently regarding the nineteenth century as a depleted vein in their country's history, have in growing numbers been reclaiming the twentieth century from amateurs and foreigners. They have concentrated especially on the two world wars, the decades between the wars, the Occupation, and the Resistance. Americans and the English continue to find the nineteenth century attractive, and they are now perhaps more active in that field than the French themselves.¹⁹ Their different points of view and methods are recognized and appreciated by French reviewers of their books. Eugen Weber's *Peasants into Frenchmen: The Modernization of Rural France, 1870-1914* (1976), Edward Shorter and Charles Tilly's *Strikes in France, 1830-1968* (1974), Robert R. Locke's *French Legitimists and the Politics of the Moral Order in the Early Third Republic* (1974), and my own *French Revolution of 1830* (1972), for examples, have been notably well received, and the monographs of a new generation now or recently appearing promise to renew significant parts of France's history in the nineteenth century; I think of the work of Ted Margadant, John Merriman, Patricia O'Brien, and a number of others.

¹⁵ Meyer, Review of William F. Church, *Louis XIV in Historical Thought: From Voltaire to the Annales School* (New York, 1976), in the *Revue historique*, 258 (1977): 198, 200, and Review of Moore, *The Revolt of the Judges: The Parlement of Paris and the Fronde, 1643-1652* (Princeton, 1971), *ibid.*, 185, 187.

¹⁶ Cairns, "Some Recent Historians of the 'Strange Defeat' of 1940," *Journal of Modern History*, 46 (1974): 71.

¹⁷ Emmanuel Todd, Interview in *L'Express* (Paris), February 17, 1979, p. 80. Author of *La Chute finale: Essai sur la décomposition de la sphère soviétique* (1976) and *Le Fou et le prolétaire* (1979), Todd is a *licencié en histoire* in France but holds a doctorate in history from Cambridge University.

¹⁸ Rémond, "La Chute de la III^e République," *Le Monde: Sélection hebdomadaire* (Paris), October 22-28, 1970, as quoted in Cairns, "Some Recent Historians of the 'Strange Defeat' of 1940," 66 n. 26.

¹⁹ See Maurice Agulhon, Review of Roger Price, ed., *Revolution and Reaction: 1848 and the Second French Republic* (London, 1975), in *Annales: Économies, sociétés, civilisations*, 34 (1979): 825.

Biography is another area that French historians have generally left to outsiders and in which Americans have won recognition in France.²⁰ A review in the *Revue historique* of two recently published American biographies of French political figures began with the unequivocal pronouncement, "Certainly, the Anglo-Saxons remain the masters in the art of biography." Jacques Godechot in his latest review article on the French Revolution and Napoleon included among ten biographies that he especially values three by Americans—Louis Gottschalk, Louis Greenbaum, and Charles Gillispie.²¹

Although Italian history has attracted relatively few American historians, their numbers have increased notably since 1945. American interest in the Italian Renaissance has long been established, and from it have come important contributions to Renaissance historiography. The newer American concern with more recent Italian history was heightened by the same conditions that stimulated expanding interest in other areas of modern European history. In addition, American intellectuals' "discovery" of the country in the postwar decades helped forge new bonds between the United States and Italy. The external interests of Italian historians, formerly concentrated on German historical scholarship, came to include American historical writing. The journal *Storia contemporanea* has a resident American editor, Philip Cannistraro, charged with assuring review in that journal of American books on Italian history and with screening article manuscripts by American scholars. The English-language *Journal of Italian History*, established in Florence in 1978, includes articles by Americans and reviews of American publications. Review articles in other journals note American books, and a goodly proportion of them are translated and published in Italy. Particular attention has been paid to American works on Italian fascism, the interpretations of outsiders removed from the conflicts and emotions of Italian politics being especially prized.²²

Serious professional study of modern Spanish history in America, save for the history of the empire, dates only from the 1950s. In 1958 Richard Herr published *The Eighteenth-Century Revolution in Spain*; it was acclaimed by historians in Spain and became—and remains—the standard book on the subject. During the 1960s the major historical writing on the nineteenth and twentieth centuries was in the English language, by Americans and Britons, and its reception by native historians was generally favorable. The books accepted as the standard works on the Falange, on anticlericalism, and on modern agricultural reform are by Americans, and two of the three on the Civil War are by Americans, the

²⁰ See Josef Konvitz, "Biography: The Missing Form in French Historical Studies," *European Studies Review*, 6 (1976): 9–20.

²¹ Pierre Guiral, Review of Philip A. Bertocci, *Jules Simon: Republican Anticlericalism and Cultural Politics in France (1848–1886)* (Columbia, Mo., 1978), and of Benjamin F. Martin, *Count Albert de Mun* (Chapel Hill, 1978), in the *Revue historique*, 262 (1979): 255; and Godechot, "La période révolutionnaire et impériale (fin)," *ibid.*, 254 (1975): 413–15, 434–36.

²² See Philip Cannistraro, "Il fascismo italiano visto dagli Stati Uniti: Cinquant'anni di studi et di interpretazioni," *Storia contemporanea*, 2 (1971): 599–622; Stefania Natale, "La Politica economica del fascismo," *Rivista di storia contemporanea*, 2 (1973): 534–55; and Raffaella Carpenetto Firpa, "Intelletuali e mass-media nell'Italia fascista," *ibid.*, 3 (1974): 356–76.

third by a Briton.²³ American books were especially valued in Spain for their foundation in research on topics forbidden by political circumstances to native historians (or neglected by them), for their detachment from domestic political quarrels, and for their perspective. Most of them were primarily synthetic and interpretive. Recently, a new generation of American scholars, taking advantage of the opening of Spanish archives, has turned to attempting archives-based monographs, but they now encounter in Spain, once a country of relatively low costs, the same problems of rising prices and a faltering dollar that handicap American scholars in northern European countries. Synthesis and interpretation may continue to be the most practical—and rewarding—kind of historical enterprise for Americans working in Spanish history.

MODERN GERMAN HISTORY stands with modern British, French, and Russian history as one of the four principal foci since 1945 of American scholarly interest in European history. Higby reported in 1929 that “a few” American historians had always been interested in Germany, but in the pre-World War I years the actual number of active professional historians writing German history in the United States and Canada was small—small compared with those writing British history and especially small compared with those American historians who in the 1960s and 1970s made Germany their area of research and interest.²⁴ In the thirty-one years since 1949 the *Historische Zeitschrift* has published reviews of 142 books on German and Austrian history by Americans. McNeill, in his study of the years 1968–78, found 276 titles on Germany and Austria, only 35 fewer than on modern French history. (In each of the next ranking areas of American scholarly attention—Italy and Spain—he found fewer than 50 for each country.)²⁵

The great surge of interest in German history began in the 1940s, set off by an extraordinary combination of circumstances unmatched in any other national area. German history, like all European history, benefited from the intense interest created by the upheaval of the War of 1939–45 and the anxieties of recovery and readjustment after the war, but Germany had its own special attraction. There the collapse of Western ideals of democracy, parliamentary government, personal liberty, and respect for the individual, on which Americans were nurtured, had been most disastrous and shocking, and German defiance of the international order and German aggression appeared to be the sources of the death and destruction that had spread around the world. Where better to start

²³ Stanley G. Payne, *Falange: A History of Spanish Fascism* (Stanford, 1961); Joan Connelly Ullman, *The Tragic Week: A Study in Anti-Clericalism in Spain, 1875–1912* (Cambridge, 1968); Edward E. Malefakis, *Agrarian Reform and the Peasant Revolution in Spain: Origins of the Civil War* (New Haven, 1970); Gabriel Jackson, *The Spanish Republic and the Civil War, 1931–1939* (Princeton, 1965); and Burnett Bolloten, *The Grand Camouflage: The Communist Conspiracy in the Spanish Civil War* (New York, 1961), republished in a rewritten and enlarged edition as *The Spanish Revolution: The Left and the Struggle for Power during the Civil War* (Chapel Hill, 1979).

²⁴ Higby, “The Present Status of Modern European History,” 7; and Meyer, *Five Images of Germany*, 13.

²⁵ McNeill, “Modern European History,” 97.

a search for understanding what had gone wrong with the world than in German history?

The opportunity for sound training and rewarding study in that field was much enriched by two fortuitous circumstances. By far the largest group of refugee historical scholars who sought new careers in the United States came from Germany and Austria, and by the late 1940s many of them were established in American universities. There they joined a growing number of native American historians of Germany in training the aspiring graduate students attracted to the study of German history. These professors and students were fortunate in having at hand for their research the vast collection of captured German documents made available on microfilm in the United States by a project sponsored by the American Historical Association. This collection almost miraculously removed one of the great obstacles to effective training in modern European historical methods and to the production of original and significant research studies.²⁶ In America the study of no other foreign national history was so happily blessed.

The young Europeans, many of them only children, who fled Europe in the 1930s and 1940s were largely German, and they formed an abundant reservoir of graduate students fluent in German and having extraordinary knowledge of Germany and the Germans and often, too, retaining ties with their native country. The roll of active American historians of Germany who followed this route to the profession and the number and quality of their books proclaim the importance of this cause of the "take-off" of German history in America. I think of Hans Gatzke, Peter Gay, Georg Iggers, Peter Paret, Fritz Stern, Klemens von Klemperer, the late Klaus Epstein, and a dozen others. The frequent incidence in the ranks of accomplished American historians of Germany of such names as Beck, Deutsch, Helmreich, Koehl, Krieger, Pflanze, Reichard, Rohr, Schorske, and Schroeder suggests that the existence of a large number of Americans of German descent, surely larger than that of any other European national group save the British, was still another source of the flourishing of German history in the United States. One rarely finds a French name among American historians of France or a Spanish name among those of Spain.

The personal ties between Germany and America through both professors and students may account, in part, for the warm reception accorded by German reviewers to books by American historians (see Table 3). Of the almost one hundred and fifty reviews of such books in the *Historische Zeitschrift* since 1949, when publication of that journal resumed after the war, 67 percent were "Favorable" or "Enthusiastic." By contrast, the *Revue historique* so favored only about half of the books by Americans that it reviewed, and the *English Historical Review* but 60 percent. *Geschichte in Wissenschaft und Unterricht* among all eleven journals sur-

²⁶ George L. Mosse, "Die amerikanische Geschichtsschreibung—Ein Überblick," *Die Welt als Geschichte*, 12 (1952): 271; John L. Snell, "Dissertationen zur deutschen Zeitgeschichte an amerikanischen Universitäten, 1933–1953," *Vierteljahrshefte für Zeitgeschichte*, 1 (1953): 289; Norman Rich, *Germany, 1815–1914*, American Historical Association Publication, Service Center for Teachers of History, no. 73 (Washington, 1968), 3; and Meyer, *Five Images of Germany*, 34–35.

TABLE 3
Modern German History: American Books Reviewed, 1945-79

Judgment	<i>Historische Zeitschrift</i> , 1949-	<i>Geschicht in Wissenschaft und Unterricht</i> , 1950-	<i>Archiv für Sozialgeschichte</i> , 1961-
ENTHUSIASTIC	6	0	0
FAVORABLE	89	43	9
NEUTRAL	12	4	1
RESERVED	16	7	7
UNFAVORABLE	19	8	4
TOTALS	142	62	21

veyed had the highest proportion of favorable reviews—69 percent. The substantial number of American books translated into German and published by German publishing houses, unmatched in France, is yet another testimony to the German profession's acceptance of American work.

The American contributions to modern German history most respected and acclaimed by reviewers have tended to be in areas neglected by historians in Germany. Leonard Krieger observed that German national historians, like the French, have neglected lost causes in their nation's past, leaving gaps into which Americans have moved with notable success.²⁷ Following the much earlier lead of Guy Stanton Ford, who published his *Stein and the Era of Reform in Prussia, 1807-1815* in 1922, William O. Shanahan, Walter Simon, Peter Paret, and Richard Raack produced books on aspects of that subject that have won favorable reception from German reviewers.²⁸ The German and Austrian revolutions of 1848-49 have inspired books by Jerome Blum, István Deák, Theodore Hamerow, Stanley Pech, and R. John Rath. The development of the German socialist and labor movements has been explored and interpreted in the works of Peter Gay, Richard Hunt, Vernon Lidtke, Carl Schorske, and a dozen others.²⁹

In the study of the history of the between-wars decades, 1919-39, American historians of Germany have enjoyed advantages comparable to those of their counterparts in French history. They can pursue it with a detachment probably unmatched by Germans and can reasonably expect to produce more balanced reconstructions of the past and, perhaps, more valid interpretations. The perspective from America, Hajo Holborn maintained, can give "many events and ideas of German history . . . their proper proportions."³⁰ The list of American historians who have written books on the Weimar Republic is long and impressive and so, too, is the list of those who have written on the Nazi period. And both lists continue to grow. The psychohistory of Adolf Hitler and other Nazis is

²⁷ Krieger, "European History in America," 306.

²⁸ For comment on American interest in this subject by a scholar from the University of Bonn, see Peter G. Thielen, Review of Paret, *York and the Era of Prussian Reform, 1807-1815* (Princeton, 1966), in the *Historische Zeitschrift*, 206 (1968): 417.

²⁹ See John A. Maxwell, "On American Studies of the German Labor Movement," *Archiv für Sozialgeschichte*, 14 (1974): 593-609.

³⁰ Holborn, *A History of Modern Germany*, vol. 1: *The Reformation* (New York, 1959), x.

largely the creation and preserve of Americans—notably Walter Langer, Rudolph Binion, Robert G. L. Waite, and Peter Loewenberg.³¹

In a different quarter of German history one can reasonably speak of an “American school.”³² “After the dissolution of the middle European community,” declared a writer in the *Historische Zeitschrift* in 1961, “the historiography of the Habsburg Empire fell into a long sleep, out of which it awoke, fresh and lively and especially active in America.”³³ German reviewers in approving, sometimes acclaiming, the books by Americans—William A. Jenks, Robert Kann, Enno Kraehe, Arthur May, R. John Rath, and several others—have observed that American historians of the Habsburg Empire enjoy the double advantage of removal from the nationalist passions of the area and of having the insights of those who have experienced life in a federal system that has worked and flourished.³⁴

As judged and accepted by historians in the country whose history they write, American historians of Germany resemble American historians of Britain more closely than their American colleagues in French history. They are accepted as equals, their research is appreciated, their interpretations are considered and respected, and their books are often translated and published in Germany. Some of their works have won praise approaching that lavished by English reviewers on a few American books on British history. Roland Bainton’s *Here I Stand: A Life of Martin Luther* was hailed by a reviewer in the *Historische Zeitschrift* as “this beautiful book.” “We have no . . . comparable book in the German language.” Theodore Hamerow’s *The Social Foundations of German Unification, 1858–1871* was described in the *Historische Zeitschrift* as “a standard work concerning the foundation of the Reich,” and the same journal’s reviewer of Gerald Feldman’s *Iron and Steel in the German Inflation, 1916–1923* praised it as a “trail-blazing achievement.”³⁵

³¹ Wolf-Rüdiger Hartmann, “Adolf Hitler: Möglichkeiten seiner Deutung II,” *Archiv für Sozialgeschichte*, 16 (1975): 586–602. Also see Peter Loewenberg, “Psychohistorical Perspectives on Modern German History,” *Journal of Modern History*, 47 (1975): 229–79.

³² Jean Berenger, “Bulletin historique: L’Empire des Habsbourg de 1525 à 1918,” *Revue historique*, 259 (1978): 180.

³³ Heinrich Benedikt, Review of Robert A. Kann, *A Study of Austrian Intellectual History from Late Baroque to Romanticism* (New York, 1960), in the *Historische Zeitschrift*, 193 (1961): 239.

³⁴ For representative reviews and literature surveys, see Hans Herzfeld, Review of Robert A. Kann, *The Multinational Empire: Nationalism and National Reform in the Habsburg Monarchy, 1848–1918*, 2 vols. (New York, 1950), in the *Historische Zeitschrift*, 175 (1953): 348–52, and Review of Robert A. Kann, *The Habsburg Empire: A Study in Integration and Disintegration* (New York, 1957), *ibid.*, 192 (1961): 668–70; Heinrich Benedikt, Review of Arthur G. Haas, *Metternich, Reorganization, and Nationality, 1813–1818: A Story of Foresight and Frustration in the Rebuilding of the Austrian Empire* (Wiesbaden, 1963), *ibid.*, 198 (1964): 686–88, and Review of Kann, *A Study of Austrian Intellectual History*, *ibid.*, 193 (1961): 239–40; Heinrich Lutz, Review of H. G. Koenigsberger, *The Habsburgs and Europe, 1516–1660* (Ithaca, N.Y., 1971), *ibid.*, 215 (1972): 408–10; Fritz Blach, Review of Richard L. Rudolph, *Banking and Industrialization in Austria-Hungary: The Role of Banks in the Industrialization of the Czech Crownlands, 1873–1914* (Cambridge, 1976), *ibid.*, 224 (1977): 740–41; Willy Andreas, “Absolutismus und Aufklärung,” *Geschichte in Wissenschaft und Unterricht*, 14 (1963): 725–26; Waldemar Besson, “Deutsche Geschichte (mit Nachbarstaaten),” *ibid.*, 16 (1965): 66–67; and Hans Herzfeld, “1877–1918,” *ibid.*, 451–52.

³⁵ Heinrich Bornkamm, Review of Bainton, *Here I Stand: A Life of Martin Luther* (Nashville, Tenn., 1950), in the *Historische Zeitschrift*, 173 (1972): 133; Elisabeth Fehrenbach, Review of Hamerow, *The Social Foundations of German Unification, 1858–1871: Struggles and Accomplishments* (Princeton, 1972), *ibid.*, 218 (1974): 450–51; and Carl-Ludwig Holtfrerich, Review of Feldman, *Iron and Steel in the German Inflation, 1916–1923* (Princeton, 1977), *ibid.*, 226 (1978): 752.

TABLE 4
Modern Russian History: American Books Reviewed, 1945–79

Judgment	<i>Voprosy Istorii</i>	<i>Istoriia SSSR, 1957–</i>
ENTHUSIASTIC	0	0
FAVORABLE	4	12
NEUTRAL	0	0
RESERVED	10	10
UNFAVORABLE	8	10
TOTALS	22	32

BEFORE WORLD WAR II A FEW AMERICANS worked and published in Russian history, but only after the war did it become a prime area of American scholarly attention. The influences of the war, the GI Bill, and expanding university enrollments were enhanced in the case of Russia by the wartime alliance and the Cold War—the Soviet Union suddenly emerged as a subject of great national concern. Yet our knowledge of the USSR, compared with that of the major Western European countries, was relatively meager. The government, the armed forces, business, and ordinary citizens wanted to know more. The success of the military's wartime foreign area study programs in training specialists in Russian language, culture, and institutions encouraged universities in the post-war years to develop their Russian offerings. Foundations were generous in their support and so, too, was the government with its NDEA and NDFL fellowship programs and institutional grants. The wartime area study programs, moreover, produced a large number of skilled and mature young men and women anxious to continue their work in Russian studies; many of them chose graduate study in Russian history.³⁶ Buoyed by this support, graduate schools in the 1950s, 1960s, and 1970s produced a broadening stream of men and women trained in Russian history, and from them and their students came an astonishing number of books. McNeill's survey found 311 books published between 1968 and 1978 on Russian history since 1750, a total exceeded among European national histories only by that for British history.³⁷

The application to American work on Russian history of the method of measuring quality by the judgments of the historical establishment in that country raises two serious problems. First, relatively few American books are reviewed in Soviet journals. Only two—*Voprosy Istorii* ("Questions of History") and *Istoriia SSSR* ("History of the USSR")—regularly review American books, and since 1945 *Voprosy Istorii*, the nearest Soviet equivalent of the *American Historical Review*, has reviewed only 22 American books on modern Russian history, and *Istoriia*

³⁶ Philip E. Mosely, "The Growth of Russian Studies," in Fisher, *American Research on Russia*, 6–10.

³⁷ McNeill, "Modern European History," 97.

SSSR, founded in 1957, has reviewed only 32 (see Table 4). Second, most areas of modern Russian history are politically sensitive in the Soviet Union, and the criteria for judgment publicly applied to books in the field are, in American eyes at least, likely to be more political than scholarly. Review articles on books by Western scholars ordinarily carry titles that include the loaded phrase "bourgeois historiography."

Not surprisingly, in my survey of Soviet reviews I found a distinctly less favorable reception of American books than that found in Western European reviews, although by no means a broad rejection. Reviewers commonly disapprove of American interpretations, but they respect and often praise American archival research, a combination of conflicting judgments that largely explains the relatively high proportion of reviews in the "Reserved" category. Two patterns in my compilation appear to illustrate this potent political element of professional judgments: (1) the number and proportion of books approved rose in the years of detente; and (2) the favorable reviews are more frequent in prerevolutionary history than in the years since 1917. Only one book on the October Revolution—and that peripherally on the subject—won clear approval.³⁸

The dilemma that I originally saw confronting American historians of modern France appears simple and clear cut in comparison with the complex choices facing American historians of modern Russia. For the latter the inclination to undertake monographs based on archival research has been tempered not only by distance and the expense of travel but also by the difficulty, sometimes even the impossibility, of obtaining permission to enter the country. Until the negotiation of formal cultural exchange arrangements in the late 1950s, American scholars simply could not get to archival materials, and even now access to particular collections can be complicated and unpredictable. But broad interpretive works based largely on materials available in the United States and Canada, which I recommended to American historians of France because of the obstacles to prolonged research in French archives, have little chance of winning approval from historians in the Soviet Union not only for ideological reasons but also because they place a high value on exhaustive research. The American books most favorably judged are those based on deep and extensive archival research. American scholars have some chance of earning the praise of their Soviet peers only if they undertake such research, as a few have successfully done (such as Terence Emmons for his *The Russian Landed Gentry and the Peasant Emancipation of 1861* [1968], Valentin Boss for his *Newton and Russia*, and Philip Pomper for his *Peter Lavrov and the Russian Revolutionary Movement*),³⁹ but they run the considerable risk of failure by being denied access to the archives they must use. Yet, if they

³⁸ G. I. Belousov, Review of Philip S. Foner, *The Bolshevik Revolution: Its Impact on American Radicals, Liberals, and Labor* (New York, 1967), in *Istoriia SSSR*, no. 5 (September-October 1968), 230-32.

³⁹ See I. P. Rakhmanova et al., "Ob osveshchenii istorii SSSR v shkol'nykh chebnikakh i posobiakh FRG," *Istoriia SSSR*, no. 3 (May-June 1969), 198-202; Iu. Kh. Kopelevich and N. I. Nevskaiia, Review of Boss, *Newton and Russia: The Early Influence, 1698-1796* (Cambridge, Mass., 1972), *ibid.*, no. 3 (May-June 1974), 212-14; and V. M. Novikov, Review of Pomper, *Peter Lavrov and the Russian Revolutionary Movement* (Chicago, 1972), in *Voprosy Istarii*, no. 9 (September 1975), 189-91.

undertake broad interpretive works, they face the probability of disapproval by their Soviet peers. A further complication is that approval by the native professionals is not always regarded in American academic circles as an accolade to be sought. More clearly than for American historians of Western and Central Europe, the most significant reading public of our historians of Russia would seem to be American and English. If this be true, then the writing of works of synthesis and interpretation must be the most rewarding scholarly activity.

American interest in the history of Eastern Europe other than Russia greatly increased after the war, and this increase depended on essentially the same forces that influenced American study of Russian and German history—the upheavals of war and revolution, the anxieties of the Cold War, and the abundant supply of fellowship and research money from foundations and governmental agencies. Here, too, the emigrants, especially the young, provided a pool of linguistically qualified and knowledgeable students. From this pool came the foreign-born but American-trained scholars István Deák, Stephen Fischer-Galati, Peter Sugar, and George Barany, who with native Americans, many with family connections in Eastern Europe (such as Wayne Vucinich, Charles Jelavich, and Leften Stavrianos), created a veritable American school of Eastern European history. In the 1950s, 1960s, and 1970s this school produced a broadening flow of books. McNeill counted 148 published in the years 1968–78 alone. Research in the field is handicapped, as in the Soviet Union, by the uncertainties of access to archives and, additionally, by the shortcomings of organization and classification of collections that are open, which probably explains why most Americans in the field have chosen quite broad subjects for their books.

It is probably impossible—certainly it is impossible for me—to generalize about Eastern European historians' reception of these books. I understand that they do value the Americans' detachment from the intense national and regional hostilities of the area that they can bring to their historical studies, and their books are praised for their outsiders' insights and interpretations.

WHAT CAN I CONCLUDE from this survey of our European peers' reception of our books on their histories? In British history Americans are part of the establishment, participants in the continuing renewal of the subject, and accepted as equal and respected colleagues. American historians of Britain, if their goals and the opportunities for publication do not drastically change, are probably well advised to continue the kind of research and publication—chiefly monographic—that has so profitably engaged them for decades. In French history some archive-based monographs have made contributions recognized by French scholars, but in French esteem none of them rivals their own great dissertations. The cost of travel to and residence in France for holders of weak dollars makes the prospect of our equaling these French models even more remote now and in the future than in the past. For the mature scholar works of synthesis and interpretation still, I believe, hold the greatest promise of professional achievement

and recognition. For historians of Italy and Spain the same advice is, I think, timely and appropriate.

The conclusion to be drawn from the generally favorable German reception of American books is less clear. One might judge that it calls for the same recommendation as for British history, but the conditions that created the extraordinary bond of sympathy between German historians and their American colleagues are passing. The generation of refugee professors who trained so many American scholars and whose association with these Americans was, in itself, a recommendation of their works is almost gone. The German-born but American-trained scholars, most of them now senior professors, and their American-born colleagues are training a generation of American historians of modern Germany who are in no way insiders like so many of their predecessors. Moreover, the availability in the United States of the massive collection of German captured documents is no longer an unequalled advantage for Americans. The documents are now available on both sides of the Atlantic, and, having been well worked over and exploited, they no longer hold the great interest that they did in the first postwar decade. The emerging generation of American historians of modern Germany may find themselves in a situation similar to that of their colleagues in French history, and the same advice may become equally appropriate for them.

Russian and Eastern European history presents American scholars with the most difficult choices, and for that very reason raises more compellingly the question, For whom are we American historians of Europe writing? or, put in more practical terms, Who reads what we write? Although we want and try to write for the international community of scholars, we are read largely by our English-speaking colleagues, students, and interested laymen. Perhaps we should heed the judgments of these readers reflected in their reviews and consider what books we as American historians of Europe judge most useful, influential, and important. No two historians' lists of such books would be identical, but I would venture to say that there would be wide agreement on such volumes as Garrett Mattingly's *The Armada* (1959), Peter Gay's *The Enlightenment: An Interpretation* (1966–69), Robert Palmer's *The Age of Democratic Revolution: A Political History of Europe and America, 1760–1800* (1959–64), and H. Stuart Hughes's *Consciousness and Society: The Reorientation of European Social Thought, 1890–1930* (1958). Ranging beyond the scope of the national monographs that have been the usual object of American scholars' endeavors, most are works of synthesis—that genre that Krieger noted as a distinctive American product in the 1920s and 1930s. Should this not be a continuing effort on the part of Americans?

We must consider, too, that national histories are likely to be less important in the decades ahead. In 1979 Theodore Zeldin remarked to a gathering of American historians of France that in fifty years studying the history of France may well be like studying the history of Texas today. Regional history may attract Europeans if movements for regional autonomy on the Continent continue and flourish, but for outsiders the field of interest will more probably be Europe. Transnational history offers rewarding subjects for our research and writing.

Many of the volumes in William L. Langer's *The Rise of Modern Europe* demonstrate how well the transatlantic scholar can illuminate the whole of European history for both Americans and Europeans. Recently some of our colleagues have given us valued models of monographs on truly European subjects—Jerome Blum's *The End of the Old Order in Rural Europe* (1978), Robert Wohl's *The Generation of 1914* (1979), and Elizabeth Eisenstein's *The Printing Press as an Agent of Change: Communications and Cultural Transformation in Early Modern Europe* (1979). They point, I think, to our best and fairest road ahead.

Noble Income, Inflation, and the Wars of Religion in France

J. RUSSELL MAJOR

IN THE MID-NINETEENTH CENTURY, Augustin Thierry argued in his famous *Essai sur l'histoire de la formation et des progrès du tiers état* that, from the revival of the free municipalities in the twelfth century until the meeting of the Estates General in 1789, the French third estate followed a "simple and regular" course in accordance with "one consistent plan" that led to the triumph of its ally, the absolute monarchy, and ultimately to the union of the three orders in June 1789. At that point "the family became complete," and for a moment the French appeared to have become a united nation. Thierry gave the clergy a nod and credited the nobility with "chivalry" and "military valor." "They knew how to die," he generously asserted,¹ but, as dying is an art that can be practiced but once, he did not assign them a very large role in determining the historical process.

Although Thierry insisted that the bourgeoisie was but one segment of the third estate, Marxist and non-Marxist historians alike soon began to depict France from the twelfth century to the Revolution as marking the rise of the middle class and a corresponding decline of the nobility. That it took the middle class so many centuries to rise and the nobility such a long time to die should have aroused the suspicions of perceptive historians. Nevertheless, when A. F. Pollard delivered his lectures entitled *Factors in Modern History* at the beginning of the twentieth century, he incorporated all of the traditional clichés. With the growth of commerce and industry, he argued, the middle class emerged and made its contribution to the establishment of the new monarchy, the founding of the British Empire, and nearly everything else that he regarded as good. "Where you had no middle class, you had no Renaissance and no Reformation," he confidently proclaimed.² At the same time the decline of the manorial system, the invention of gunpowder, the growth of nationalism, the emergence of absolute monarchies, and so on led to the decline of the nobility.

I would like to express my appreciation to Emory University, to the Institute for Advanced Study, Princeton, and to the National Endowment for the Humanities for the support that enabled me to write this article during 1979–80. I have benefited from conversations with John Elliott, Alexander Field, and John H. M. Salmon concerning certain problems my data presented, but the conclusions are my own.

¹ Thierry, *Essai sur l'histoire de la formation et des progrès du tiers état* (3d ed., Paris, 1855), viii, 188, xii. Thierry formulated his basic argument in the 1820s and published a version of his views in the *Revue des deux mondes*, May 1846, pp. 521–48, June 1846, pp. 722–41, March 1850, pp. 813–42, and May 1850, pp. 469–89.

² Pollard, in his *Factors in Modern History* (New York, 1907), 41.

Even fifty years ago, when Marc Bloch discovered that there was a seigneurial reaction following the Hundred Years War, he attributed it primarily to the middle class, who purchased land from noble and peasant alike. They, and not the nobility, took the lead in introducing bookkeeping and more advanced administrative methods into the countryside and in acquiring peasant holdings in order to re-create large estates.³

Today our admiration for the middle class has dissipated, and there would be little point in recalling the works of such historians as Thierry and Pollard had not their interpretation of noble irrelevance and inability to cope with changing circumstances persisted. The typical historian, with a mortgage on his house, payments due on his car, and several credit cards in his pocket still recoils in discovering that a noble borrowed money to remodel his chateau. If a noble provided his daughter with a larger dowry than his father had for his sister, it is taken as proof of fiscal irresponsibility rather than increased affluence. Such presumptions must be expelled from our minds if we are to understand the Renaissance nobility and approach its activities as we would those of other segments of society.

Robert Boutruche, the first of the post-World War II rural historians, laboriously catalogued the factors that reduced the nobility to financial impotence during the Hundred Years War. Devastation caused by that conflict, debasement of the currency, breakup of the manorial system, higher labor costs, and dismemberment of the patrimony among too numerous children reduced family income. At the same time, war, extravagance, and pious gifts conspired to keep noble expenses high. Boutruche recognized that some families sought to mend their fortunes, occasionally with success, but the financial picture of the nobility that he painted was bleak.⁴ Subsequently, a number of local studies have been published that seem to confirm his findings.⁵ Here and there someone has noted that the nobility partially recovered during the peaceful years of the Hundred Years War or that nobles did not fare too badly in regions that escaped the worst of the war, such as parts of Brittany and Auvergne,⁶ but on the whole recent studies have reinforced the idea of noble decline, an impression that has been incorporated into surveys of the period.⁷

In 1966 French rural history took a great leap forward when Emmanuel Le Roy Ladurie published a study of the peasants of Languedoc; eleven years later

³ Bloch, *Les Caractères originaux de l'histoire rurale française* (Oslo, 1931), chap. 4, sec. 3.

⁴ Boutruche, *La Crise d'une société: Seigneurs et paysans du Bordelais pendant la Guerre de Cent Ans* (Paris, 1947).

⁵ See, for example, Guy Fourquin, *Les Campagnes de la région parisienne à la fin du Moyen Âge* (Paris, 1964); Isabelle Guérin, *La Vie rurale en Sologne aux XIV^e et XV^e siècles* (Paris, 1960); Marie-Thérèse Lorcin, *Les Campagnes de la région lyonnaise aux XIV^e et XV^e siècles* (Lyons, 1974); Emmanuel Le Roy Ladurie, *Les Paysans de Languedoc*, 2 vols. (Paris, 1966); Guy Bois, *Crise du féodalisme* (Paris, 1976); André Plaisse, *La Baronnie du Neubourg* (Paris, 1961); and J. Tricard, "Les Limites d'une reconstruction rurale en pays pauvre à la fin du Moyen Âge: Le Cas du Limousin," *Études rurales*, 60 (1975): 5-39.

⁶ Pierre Charbonnier, *Guillaume de Muro: Un Petit seigneur auvergnat au début du XV^e siècle* (Clermont-Ferrand, 1973), 243-50; J. Kerhervé, "Le Domaine ducal de Guingamp-Minibriac au XV^e siècle: Étude de comptes," *Mémoires de la société d'histoire et d'archéologie de Bretagne*, 55 (1978): 123-83; and Monique Chauvin-Lechaptois, *Les Comptes de la châtellenie de Lamballe, 1387-1482* (Rennes, 1977).

⁷ Emmanuel Le Roy Ladurie, "Les Masses profondes: La Paysannerie," in F. Braudel and E. Labrousse, eds., *Histoire économique et sociale de la France*, 1, pt. 2 (Paris, 1977): 523-29.

he expanded his treatment to include all of France. In his hands population changes became the principal determinant of the historical process. Disease and war, he argued, reduced the population by about 50 percent between 1300 and 1450. The scarcity of labor compelled landlords to lower rents both in money and in kind, to reduce seigneurial dues, and to increase wages in order to attract and retain tillers of the soil. At the same time, declining demand reduced the prices of the products they had to sell. The result was a golden age for peasants and hard times for the lords of the manor. Then the situation was reversed. Between 1450 and 1560 the population doubled. Food prices soon began to rise rapidly because agricultural production failed to meet the needs of the growing population. Under such circumstances, the owner of a fief who exploited the domain himself, leased it for payments in kind, or used the sharecrop system could maintain his position. The owner who relied largely on monetary leases and dues paid in money continued to slide toward disaster. During the last four decades of the sixteenth century the Wars of Religion provoked a new crisis. Agricultural production dropped by about one-third, primarily because of the direct damage done by troops and the loss of livestock that reduced the amount of manure for fertilizer. This decline quickened inflation, although the population remained relatively stable.⁸ This period of rapid inflation has generally been considered especially disastrous for the nobility.

The achievement of Le Roy Ladurie and other scholars of the *Annales* school has been remarkable; but they have failed to consider one aspect of the nobles' situation at the close of the Hundred Years War, and they have given us very little specific information about how the nobility fared during the inflation-ridden, war-torn sixteenth century. Unquestionably, the *Annales* historians' belief that the economic position of most noble families deteriorated drastically during the Hundred Years War is correct, but this deterioration was but one part of the picture. Just as the peasant had enlarged his holdings by acquiring the lands of deceased relatives during the period of depopulation, so the noble must have increased the number of his fiefs. Often he was able to incorporate vacated lands. In the county of Bigorre, for example, there were forty fiefs in 1313 but only eighteen in 1429. Twelve fiefs had disappeared because the villages on which they depended had ceased to exist, although the land, of course, remained. Ten fiefs had been acquired by six of the surviving eighteen seigneurs.⁹ For the time being these eighteen seigneurs were in a difficult economic position, but they held what forty had shared only a century earlier. By waiting a generation or two for the rising population to bring down labor costs and raise rents and grain prices, would they not then have begun to prosper? This essay will test this hypothesis and ascertain how the nobility fared when the population stabilized and inflation worsened during the Wars of Religion. In making this study, I will rely on the financial records of the house of Foix-Navarre-Albret and available published evidence.

⁸ Le Roy Ladurie, *Les Paysans de Languedoc*, and "Les Masses profondes," 483-865.

⁹ Maurice Berthe, *Le Comté de Bigorre: Un Milieu rural au bas Moyen Âge* (Paris, 1976), 130-34. Some of the families of the eighteen seigneurs had not been in Bigorre in 1313.

WITH THE EXCEPTION OF THE VALOIS DUKES OF BURGUNDY, no French noble family accumulated as many fiefs during the late Middle Ages as did the house of Foix. Through marriage, purchase, and war the family acquired one lordship after another in southern France, including the kingdom of Navarre and the independent viscounty of Béarn. The problem of the counts of Foix was not to provide for a multitude of children in each generation but to produce an adult male heir. The demographic disasters of their age provided both a splendid opportunity to acquire more land and a serious threat to their existence. At length they, too, failed to produce a son, and in 1484 the daughter of the house married Jean d'Albret, heir to extensive lands in the southwest. The fiscal administration of this couple's descendants are largely the concerns of this essay.

The family owed its fiscal success during the Renaissance largely to Henri II d'Albret (1517–55). Henri did not need the bourgeoisie to tell him how to manage his estates. His grandmother's ancestors, the counts of Foix, had had their treasurers keep detailed records of their receipts and expenses as far back as the fourteenth century, and by 1460 their treasurer in the viscounty of Lautrec was totaling the entries at the bottom of each page in Arabic numerals.¹⁰ For administrative supervision Henri turned to the crown for a model. In 1520 he established a *Chambre des Comptes* at Pau to audit the accounts of the financial officials in his numerous domains and to perform other duties. In 1527 he reduced its jurisdiction by creating a *Chambre des Comptes* at Nérac to supervise Albret, Armagnac, Périgord, Limousin, Rouergue, Fézensaguet, Foix, Nébouzan, Lautrec, and Villemur. Some time after his daughter, Jeanne d'Albret (1555–72), married Antoine de Bourbon, Vendôme and other fiefs in the north were temporarily added to its jurisdiction. The *Chambre des Comptes* at Pau retained responsibility for Béarn, Navarre, Bigorre, Marsan, Tursan, and Gavardan. In the course of time many records have been lost, but no less than four thousand *cahiers* and *cartons* containing documents once held by the two *Chambres* still existed when an inventory of their contents was published in 1863. They provide one of the finest opportunities to study the financial position of a great noble family during the latter half of the sixteenth century.¹¹

Henri d'Albret divided his counties, viscounties, and other domains among a number of treasurers, each of whom administered one or more of these jurisdictions. Each treasurer collected the money that was due from his jurisdiction, paid local expenses, and periodically turned over the surplus to a general treasurer. The accounts of all of the treasurers and of the officials who expended sums to support the royal household were verified annually by the appropriate *Chambre des Comptes*.

¹⁰ For the medieval receipts and expenses of the viscounty of Lautrec, see Archives départementales, Pyrénées-Atlantique [hereafter, AD-PA], B1733–B1737. Unfortunately, Roman numerals soon replaced Arabic numerals in the page totals. For medieval accounts of other fiefs, see AD-PA, B1763–B1772, and series E, which was not part of the *Chambres des Comptes*.

¹¹ P. Raymond, ed., *Inventaire-sommaire des archives départementales antérieures à 1790: Basses-Pyrénées*, 1 (Paris, 1863): 10. There were some changes in the jurisdictions that the two *Chambres* administered after 1527. A few documents were destroyed or lost in a fire in 1908. I have only been able to sample the vast collection that remains. There is more than enough to support a good doctoral dissertation or, for that matter, a substantial book.

There are difficulties in using these records. First, no one account summarizes all of the revenue the family received or all of the money that it spent. Gifts and salaries paid by the king of France do not always appear, and military expenses, except for the garrison in the powerful fort of Navarrenx and the salaries of a few captians, are not included in the dominal and household accounts (special treasurers dealt with troops during the Wars of Religion). Second, and perhaps more serious, treasurers kept records in order to account for the money that passed through their hands rather than to record profits and losses. Hence, money that remained in a treasurer's hands from the preceding fiscal year is listed as a receipt, and money turned over to the Albrets' general treasurer is counted as an expense. Money that was borrowed or derived from the sale or alienation of property and money that was transferred in from another account were also counted as receipts, just as the payment of a debt was entered as an expense. In ordinary times, the error incurred by considering the receipts from a domain as gross income is not large, and a comparison of the total receipts for a number of years quickly reveals the upward or downward trend of the gross yield. During the Wars of Religion, however, so much land was sold or alienated in some jurisdictions and so much money was transferred in and out of some accounts that the actual revenue can be ascertained only after an exhaustive study, if at all. The total "expense" is of no value, because it includes money transferred to the general treasurer and money spent at the direction of the Albret family that had nothing to do with the administration of the jurisdiction—that is, the total "expense" includes the net income.

The best surviving series of accounts is that for the independent viscounty of Béarn. Here the stewards (*baylies*) and notaries turned over, after deducting their expenses, the sums they collected from the individual subordinate jurisdictions to the treasurer of Béarn. Mills were leased for six years, but the tolls were farmed for more limited periods or collected by local officials. Careful management brought ever-increasing revenue until Béarn itself was invaded during the Wars of Religion. Officials were evidently proud of their achievement, for one of them recorded in 1539 that revenue from the domain had been increased by 2,000 *écus* "without doing an injustice to anyone."¹² Gross receipts that in 1530–31 stood at 12,856 Béarnais *écus* reached 38,461 *écus* in 1564–65, a threefold increase in thirty-four years (see Table 4, below).¹³

Receipts from the Albret domains in Périgord and Limousin increased from 10,566 *livres* in 1550–51 to 16,762 *livres* in 1573–74,¹⁴ and their county of Fézensaguet, whose gross receipts were 1,654 *livres* in 1561–62, was leased for 2,538 *livres* in 1569–70.¹⁵ The treasurers of the southern fiefs—except those of Béarn, Navarre, and Foix—turned over 65,952 *livres* to the treasurer of the royal

¹² AD-PA, B246, as quoted in Charles Dartigue-Peyrou, *La Vicomté de Béarn sous le règne d'Henri d'Albret, 1517–1555* (Paris, 1934), 250.

¹³ For an account of the administration of the domain, see Dartigue-Peyrou, *La Vicomté de Béarn*, 245–53.

¹⁴ AD-PA, B1827, B1862. The fiscal year began and ended on Saint John the Baptist's Day.

¹⁵ AD-PA, B1582, B1588. The fiscal year began and ended on Saint John the Baptist's Day.

TABLE 1
Receipts and Expenses in Navarre

<i>Transactions</i> ^a	<i>1553</i> ^d	<i>1555</i> ^d	<i>1568</i> ^d	<i>1575</i> ^d
RECEIPTS FROM THE				
PRECEDING YEAR	200	1,425 ^e	3,064 ^f	53
DOMAIN	5,017	4,478	1,781	4,800
ESTATES (DONATION) ^b	4,486	3,925	0	15,021
TOTAL RECEIPTS	9,703	9,828	4,845	19,874
PAYMENTS				
TO THE GENERAL TREASURER ^c	2,012	2,025	1,383	9,114
TOTAL PAYMENTS	7,542	9,137	4,787	19,130
<i>AD-PA Source</i>	B1412	B1414	B1415	B1417

^a Figures for all of the transactions are in *livres tournois*. Payments in kind have been omitted.

^b The donation voted by the estates was turned over to the treasurer of Navarre and included in his accounts. The estates made no donation in 1568.

^c Payments to the general treasurer or other officials of the crown of Navarre were carried as expenses, but they represent a substantial part of the royal "profits." Some other payments were also made to persons not directly employed in Navarre and were, therefore, also part of the "profits."

^d The fiscal year in Navarre was from January 1 to December 31, even before the adoption of the Gregorian calendar.

^e The treasurer held 1,000 *livres* from the preceding year to pay debts contracted during that year.

^f A large Catholic uprising in the fall of 1567 was not suppressed until the following year. A large sum was apparently transferred into the treasurer's account or left in his account in anticipation of a decline in revenue in 1568. In spite of this precaution, pensions and judges' salaries went unpaid.

household in 1556 and 85,039 *livres* in 1560.¹⁶ There were, of course, occasional exceptions to the general upward trend prior to the most destructive stage of the Wars of Religion. Much of the domain in Navarre yielded fixed monetary payments, and an exceptionally high proportion of the receipts were derived from tolls. The result was a relatively static situation there, except when a rebellion reduced the receipts in 1568 or when the provincial estates gave more or less than their customary amount (see Table 1). Receipts from Armagnac appear to have stabilized, at least temporarily, before the Wars of Religion began, but destructive troops and alienations must have contributed heavily to the failure of the county to resume its growth during the latter part of the century (see Table 2).

The Albrets did not directly exploit the domain, nor did they rely heavily on the growing practice of turning their land over to sharecroppers. The payments they received in kind were of little importance. Instead, they preferred monetary leases, but, since these leases were nearly always for three years, their prices leaped upward with the demand for land; land rents rose even faster than grain

¹⁶ AD-PA, B6, B8.

prices. The Albrets broke up their domain into segments that were small enough to permit aspiring peasants to compete for leases, a practice that led to more competitive bidding than there would have been had they catered to the affluent. In 1531, those interested in leasing domain lands in the viscounty of Lautrec were summoned to Carcassonne, but by 1549 the Albrets had realized that competition for farms would be greater if the auctions were held in various parts of the viscounty itself. The viscounty was therefore divided into eleven parts, and the approach of Albret agents to this or that segment of the domain was widely advertised.¹⁷ The domain in the viscounty of Fézenzaguet was divided into a dozen or so segments before it was leased. Even so, two, and more often three or four, persons had to join together to make a successful bid in 1562, in 1565, and doubtless in other years. With rare exceptions the bidders were men without any form of title, not even that of judge, notary, or bourgeois of a small town. It is striking that only one group that obtained a parcel of the domain in 1562 managed to do so again in 1565. Had the demand for land forced land prices so high that the lessees of 1562 could not compete three years later, or had they learned their lesson? More likely, the price of land skyrocketed, because nearly every parcel of the domain was leased for more in 1565 than it was in 1562. In several instances, the rent more than doubled.¹⁸

Any attempt to study seigneurial revenue during the Wars of Religion is fraught with difficulties and dangers. On the one hand, the seigneur's need to borrow money, to sell or alienate part of his domain, and to transfer funds into an account was sometimes compelling. Since each of these transactions was considered a receipt, revenue seemingly rose rapidly at those times when the seigneur was in financial need. On the other hand, his revenue was likely to fall during the following years because his domain was smaller, although the yield from the remaining individual parts may have been larger. Then, too, the actual damage done by troops could obscure the secular trend. Of the lands of the house of Albret, the domain in Béarn provides the clearest picture of what would probably have happened had there been no war. The sovereign of the little state could not sell or alienate his domain without violating the privileges of his subjects, and its geographical location was removed from the center of the conflict. Here the receipts in 1584–85 were five times those of 1530–31 and over twice those of 1561–62, just before the Wars of Religion began. The one exception to the general upward trend occurred in 1568–69, when the conflict reached Béarn itself (see Table 4, below).

THE FIRST OF THE WARS OF RELIGION began in March 1562 and ended in March 1563. Jeanne d'Albret remained officially neutral, but her French fiefs were subject to the ravages of troops.¹⁹ Military action was probably greater in the terri-

¹⁷ *Ibid.*, B1741.

¹⁸ *Ibid.*, B1583, B1585.

¹⁹ For the war, see A. de Ruble, *Jeanne d'Albret et la guerre civile* (Paris, 1897); and Nancy L. Roelker, *Queen of Navarre, Jeanne d'Albret* (Cambridge, Mass., 1968), 186–207.

TABLE 2
Revenue Received by the Treasurer of the County of Armagnac^a

<i>Jurisdiction</i>	1555-56 ^b	1558-59	1562-63	1569-72 ^f	1574-75	1581-82	1595-96
Lectoure	2,385	2,470	2,666	2,606	2,850	3,279	2,197
Aurillac	2,840 ^c	1,817 ^c	2,769 ^c	2,950 ^c	1,887 ^c	4,624 ^{cs}	2,010 ^c
Lomagne	1,300	1,200	1,241	1,500	1,516	1,328	1,258
Bruilhois	2,100	1,507	1,533	1,785	1,218	957	732
Fzensac	2,305	2,275	2,676	3,500	4,535	4,228	4,681
Pardiac	3,093	2,703	3,135	3,692	3,800	3,910	2,209
Eauzan	800	852	862	1,110	505	1,050	500
Rivière-Basse	435	468	481	610	675	597	300
Astaffort	122	124	75	100	161	— ^h	195
Tournon	310	333	375	380	610	508	450
L'Isle Jourdain	5,103	5,611	4,931	5,504	6,163	3,450	4,997
TOTAL	20,793	19,360	20,744	23,737	23,920	23,931	19,529
TOTAL RECEIPTS	26,059	— ^d	29,474 ^e		30,019	26,856	28,110 ⁱ
<i>AD-PA Source</i>	B2135	B1581	B1584	B1588	B 1591	B1597	B1621

^a All receipts are in *livres tournois*.

^b The fiscal year was from one Saint John the Baptist's Day until the next.

^c Aurillac also had an annual payment of 12 *pipes* of wine, which was used to pay part of the salary of an official.

^d Total receipts are not available; some pages of the account are missing.

^e Total receipts for 1652-63 include a 6,289 surplus from the preceding account.

^f Figures represent the amount for which the domain was leased for the three-year period beginning in 1569.

^g One payment of 4,047 *livres* was for the preceding year as well as for 1581-82.

^h Astaffort was not farmed in 1581-82; the king of Navarre had assigned its revenue to one of his captains.

ⁱ Total receipts for 1595-96 include 7,906 *livres* that were held back from the account of the preceding year to meet expenses.

tory that the treasurer of Armagnac and Fézenzaguet administered. Here the land had been leased for three years beginning on Saint John the Baptist's Day in 1560. Hence, the war did not influence the amount for which the domain was leased, but it could have affected the ability of the lessees to meet their obligations. The total receipts from Fézenzaguet in 1562–63 came to 1,557 *livres*, only 93 *livres* less than in the preceding year of the lease.²⁰ A comparison of the receipts in 1562–63 with those of 1558–59 during the previous lease period reveals that, of the eleven fiefs into which Armagnac was divided, nine registered increases and only two registered losses, although the net gain came to only 1,384 *livres*. Especially surprising is Lectoure's gain, since the town was besieged and taken by the Catholics in one of the war's most important engagements in southwestern France (see Table 2).

Four years of nominal peace followed, but Jeanne's attempt to impose Protestantism led many of her Catholic subjects in Béarn and especially Navarre to rebel during the second religious war, which lasted from September 1567 to March 1568. Revenue from Navarre declined disastrously. The estates did not meet to vote their customary donation of over 4,000 *livres*; mills in the hands of the rebels yielded no income, and some tolls could not be collected. In all, Jeanne grossed less than 1,800 *livres* from her little kingdom instead of 8,500–9,500 *livres*, and this small sum had to be used to pay for repairs and other expenses. Her judicial officials and many pensioners went unpaid (see Table 1, above).²¹

Jeanne soon suppressed the rebellion, but her Catholic subjects remained discontented. Furthermore, the peace that followed between the rival factions was precarious at best. Under such circumstances, Jeanne realized that she could not hope to remain neutral and, at the same time, protect her lands when the next war began. She therefore decided to throw in her lot with her coreligionists in France, and in August 1568 she set out for La Rochelle. Charles IX retaliated on October 18 by ordering the seizure of all of her lands, and he dispatched the Baron of Terride to make good the royal threat. Terride entered Béarn on March 28, 1569. Pau quickly fell, and on May 24 he lay siege to the powerfully fortified town of Navarrenx. With Béarn almost entirely in his hands, Terride reorganized the government to insure Catholic control, confiscated the goods of the Protestants who had fled, and held a meeting of the estates to vote money to support his regime. On July 10, Jeanne countered by commissioning the Count of Montgomery to reconquer her lands, and by August 22, in a brilliant campaign, he had regained possession of Béarn.²²

The financial impact of the two invasions was profound and clearly demonstrates what often happened when one side or the other took possession of a territory during the Wars of Religion. Terride proceeded to milk the viscounty of all that he could during the five months that he held sway. The domain lands,

²⁰ AD-PA, B1582, B1583.

²¹ *Ibid.*, B1415.

²² Roelker, *Queen of Navarre, Jeanne d'Albret*, 268–71, 291–301; Pierre de Salefranque, *Histoire de l'hérésie de Béarn*, ed. V. Dubarat (Pau, 1929), 125–63; J. Russell Major, *Representative Government in Early Modern France* (New Haven, 1980), 249–50; and Nicolas de Bordenave, *Histoire de Béarn et Navarre, 1517 à 1572*, ed. P. Raymond (Paris, 1873), 150–290.

mills, and tolls yielded some money, and he pressured the towns and estates for substantial amounts. In all, he extracted over 18,000 *livres* from the war-torn viscounty, while apparently contributing nothing to the expenses of governing it.²³

Following his victory, Montgomery restored Jeanne's loyal officials, including her treasurer of Béarn. Inasmuch as the fiscal year had begun on November 1, 1568, the leases and other arrangements had been made before Terride's invasion in March of the following year. Hence, the treasurer's problem was to collect the amounts that were due, but this proved to be impossible because the viscounty had been so thoroughly fleeced. With diligence he managed to gather 25,238 Béarnais *écus*, a third less than had been collected in 1564–65, and the estates did not meet again to vote the customary donation. Furthermore, large sums had to be expended to repair the mills and other facilities. As a result, Jeanne was able to draw only 6,365 Béarnais *écus* from the viscounty—that is, about a fourth of what she had received in 1564–65 (see Table 4, below).²⁴ Jeanne's victory did, however, enable her to confiscate the goods of the Catholic Church, which yielded about 80,000 *livres* annually. A conscientious Protestant, she used part of this sum to support the ministry and to provide salaries and student fellowships for the academy she had established, but she used funds not needed for these purposes to pay some of her officials, to strengthen the fortifications at Navarrenx, and to support the war effort.²⁵

The invasion of her lands not only subjected Jeanne to a loss of revenue, but the need to support the Protestant army greatly increased her expenses. To raise money, she pawned her jewels and alienated part of her domain in Armagnac with the right of redemption (*rachat perpétuel*). The viscounty of Bruilhois brought her 10,000 *livres*, although she retained the rights of justice and the tolls. In such large transactions, noblemen often profited rather than villagers, who could only afford to bid for the right to lease small parcels of land. As a result of the alienations, Jeanne leased her domain in Armagnac for the three-year period beginning in 1569 for an amount approximately equal to what she could expect to receive in a single normal year (see Table 2, above). The civil war may have reduced the peasants' desire for land somewhat, but not enough to prevent them from offering a total of 2,538 *livres* to rent the domain in Fézenzaguët in 1569–70 and 2,507 *livres* per year in the two years that followed. In 1565–66 the total receipts of the viscounty had been 2,341 *livres*. Thus, in spite of the war, land rents were being forced upward.²⁶

The third war ended in August 1570, but the conflict was renewed following the Massacre of Saint Bartholomew's Day two years later and continued, except for relatively short periods of peace, until the close of the seventh war in Novem-

²³ Terride evidently appointed Arnaud d'Esquille, who was treasurer of Navarre in 1568, treasurer of Béarn. Esquille was probably one of the Catholics who accepted the new regime, but, when his parents had his accounts verified by the *Chambre des Comptes* in 1576 following his death, they insisted that he had acted under duress. AD-PA, B258.

²⁴ AD-PA, B254, B257.

²⁵ Charles Dartigue-Peyrou, *Jeanne d'Albret et le Béarn d'après les délibérations des États et les registres du Conseil Souverain, 1555–1572* (Mont-de-Marsan, 1934), xcvi.

²⁶ Roelker, *Queen of Navarre, Jeanne d'Albret*, 311; and AD-PA, B1588.

ber 1580. Then in 1585 the war resumed and lasted until 1598, but mercifully peace was restored in the southwest in 1594. Under such conditions the financial drain on Jeanne until her death in 1572 and on her son, the future Henry IV of France, was almost continual. Béarn, which escaped direct involvement in any of the later wars, produced ever increasing revenue for its sovereign (see Table 4, below), but Armagnac, which lay in the path of the armies, was less fortunate. In 1574–75 and again in 1581–82, periods of relative peace, receipts compared favorably with earlier years. In 1595–96, the first year of peace in the region, the receipts from the eleven jurisdictions into which the county was divided actually dropped to prewar levels because of alienations. In fiefs such as Fezensac and Astaffort—in which there were few, if any, alienations—receipts had approximately doubled (see Table 2, above).²⁷

An illuminating though somewhat misleading way to evaluate revenue is to consider the funds the general treasurer received from the great fiefs. Such receipts are misleading in two ways. On the one hand, expenses incurred by Jeanne and Henry were often paid by the treasurer of this or that county, a practice that reduced the amount available for payment to the general treasury. On the other, alienations could provide an abnormally large receipt for the year in which they took place and cause reduced revenue thereafter, until the domain was redeemed. These practices became more common in the later stages of the religious wars.

The first religious war (1562–63) evidently had little adverse effect on Jeanne's income. The receipts from her domain and the estates of Béarn that were turned over to her general treasurer came to 171,850 *livres*, as opposed to 127,315 *livres* in 1557–58. This growth is all the more remarkable in that the earlier account covered fifteen and a half months. Jeanne's decision to participate in the second, third, and fourth religious wars caused a sharp decline in receipts to 104,053 *livres* in 1572, primarily because she had been forced to alienate all or part of her domain in some counties, and the estates of Béarn did not meet to vote a donation. In 1575 and 1580 the accounts of the general treasurer included only that part of the domain that reported to the *Chambre des Comptes* at Pau, but the domain under its jurisdiction reported continual gains. There is no extant account of the revenue derived from the part of the domain that was under the jurisdiction of the *Chambre des Comptes* of Nérac. It did yield some revenue, but Jeanne and Henry preferred to alienate the domain in this region rather than their lands further south, probably because it was more exposed. As early as 1566, the seigneurie of Peysac in the jurisdiction of the treasurer of Périgord and Limousin had been alienated to the benefit of the Bishop of Poitiers for 10,000 *livres*, and it still had not been redeemed in 1574. At least two other seigneuries had been surrendered to noblemen in the same region by 1573–74 in return for 10,000 *livres* each, and other lands were also alienated (see Table 3).²⁸

²⁷ In the region south of Paris, rents in money also appear to have risen during the relatively peaceful years but often declined when the wars were at their worst; Jean Jacquart, *La Crise rurale en Île de France, 1550–1670* (Paris, 1974), 217.

²⁸ AD-PA, B1862; and Raymond, *Inventaire-sommaire des archives départementales*, 154–56.

TABLE 3
Receipts of General Treasurer of Navarre, Béarn, and Other Domains

<i>Jurisdiction</i>	1557-58 ^b	1564	1572	1575	1580	1585	1589	1595	1600	1605
Navarre	1,121	2,500	1,236	9,742	13,271	1,578	3,338	3,000	1,800	8,164
Béarn	10,076	36,642	46,295	43,577	49,356	56,564	38,901	25,719	26,181	41,300 ^a
Estates of Béarn	19,440	19,410	— ^c	25,920	34,560	30,240	20,520	19,440	15,120	17,280
Foix & Pamiers	21,583	21,789	14,802	7,424	10,535	20,612	9,246	6,000	8,919	9,130
Bigorre & Barbazan	14,268	11,538	1,678	10,983	19,087	11,847	9,999	7,998	4,800	12,910
Nébouzan & Aspet	871	3,405	4,047	2,862	6,401	4,112	3,999	1,998	—	3,703
Marsan	6,571	4,486	2,552	6,150	6,196	7,677	8,178	2,832	5,280	9,362
Albret	19,265	20,110	15,580 ^d	— ^g	— ^g	43,750	18,231	—	—	25,622
Armagnac	9,906	21,346	12,573 ^e	— ^g	— ^g	19,240 ⁱ	11,543	—	—	13,434
Fézensaguet	647	1,882	900	— ^g	— ^g	2,830	2,083	—	—	1,144
Rodez	13,421	15,381	— ^f	— ^g	— ^g	10,130	—	—	—	14,333
Périgord & Limousin	8,583	10,476	4,390	— ^g	— ^g	17,053	2,419	2,936	—	—
Lautrec & Villemur	1,623	2,885	— ^f	— ^g	— ^g	240	—	—	—	700
TOTAL	127,315	171,850	104,053	106,658	139,406	225,873	128,457	69,923	62,100	157,082
TOTAL RECEIPTS ^a	151,247	191,520	118,762	136,477 ^h	148,600	628,539 ^j	466,450 ^k	246,687 ^m	489,539 ^o	342,016 ^r

EXPENSES	150,060	197,615	127,914	121,872	144,800	624,884	— ¹	339,290 ⁿ	492,676 ^p	298,774 ^s
AD-PA Source	B143	B146	B148	B150	B155	B159	B163	B168	B171	B175

^a Total receipts include revenue from sources other than those listed above. All receipts and expenses are in *livres tournois*.

^b Because of the death of the treasurer, this fiscal year was from July 13, 1557 to November 1, 1558. All other years were from January 1 to December 31.

^c The estates of Béarn did not vote a "donation" in 1572.

^d The income from Albret in 1572 was low because the revenue from renting the mills had been alienated.

^e The estates met on November 19, 1572 and voted 25,000 *livres*. Of this sum, 3,605 *livres* had been collected and were included in this total.

^f There was no revenue from Rodez or from Lautrec and Villemur in 1572; perhaps it had been alienated.

^g Receipts from the domain under the jurisdiction of the *Chambre des Comptes* at Nérac were not included in the account.

^h Total receipts in 1575 included 16,000 *livres* from the Catholic clergy.

ⁱ The income from Armagnac in 1585 included a gift of the estates.

^j Total receipts in 1585 included revenue from Vendôme and other lands in the north, loans, and some alienations in the south.

^k Total receipts in 1589 included revenue from the domain in the north, the admiralty of Guyenne, and other sources.

^l Some pages of the manuscript are missing for 1589, including those containing the total expenses.

^m Total receipts for 1595 included small sums from northern domains, over 40,000 *livres* in sales and alienations, and 111,945 *livres* carried over from the preceding year to meet expenses that had been incurred but not paid.

ⁿ Expenses for 1595 included 137,362 *livres* in unpaid expenses.

^o Total receipts for 1600 included small sums from northern domains, over 300,000 *livres* in alienations and the sale of wood, and 68,324 *livres* carried over from the preceding year to meet expenses that had been incurred but not paid.

^p Expenses for 1600 included 256,085 *livres* used to pay creditors.

^q Receipts from Béarn in 1605 included 10,000 *livres* from the Catholic clergy.

^r Total receipts in 1605 included about 30,000 *livres* from northern domains and substantial alienations.

^s Expenses for 1605 included 87,278 *livres* used to pay creditors.

TABLE 4
Price Indices and Receipts from the Domain in Béarn

Year	Paris Wheat Indices ^a	Toulouse Wheat Indices ^b	Toulouse Wine Indices ^c	Fiscal Year ^d	Béarn Domain Indices ^e	Béarn Domain Receipts ^f	AD-PA Source
1526-35	1.00	1.00	1.00	1530-31	1.00	12,856	B225
1533-37	0.83	0.73	0.72	1534-35	1.07	13,797	B229
1538-42	0.97	1.01	0.96	1539-40	1.59	20,388	B234
1543-47	1.41	0.87	1.10	1544-45	1.68	21,644	B238
1548-52	1.39	0.83	0.62	1550-51	1.78	22,907	B242
1553-57	1.21	1.44	0.76	1554-55	1.97	25,347	B246
1558-62	1.76	1.25	0.88	1561-62	2.31	29,678	B251
1563-67	2.60	2.07	1.41	1564-65	2.99	38,461	B254
1568-72	2.60	1.95	0.72	1568-69	1.96	25,238	B257
1573-77	3.56	3.13	1.40	1575-76	3.31	42,500	B262
1578-82	2.47	2.55	0.91	1579-80	3.24	41,648	B265
1583-87	4.45	2.86	0.94	1584-85	5.00	64,249	B271
1588-92	7.67	3.35	1.98	1589-90	7.09	91,067	B279
1593-97	6.12	3.87	1.23	1594-95	6.19	79,594	B286
1598-'02	3.23	3.08	1.23	1600	7.86	101,040	B294

^a Calculated August through July; derived from Micheline Baulant and Jean Meuvret, *Prix des céréales extraits de la mercatique de Paris, 1520-1698*, 1 (Paris, 1960): 243.

^b Calculated for the calendar year; derived from Georges Frêche and Geneviève Frêche, *Les Prix des grains, des vins, et des légumes à Toulouse, 1486-1868* (Paris, 1967), 85-87.

^c Calculated for the calendar year for local red wine; derived from Frêche and Frêche, *Les Prix . . . à Toulouse*, 118-21.

^d November 1-October 31; in 1600 the treasurer used the calendar year, but local officials continued to use the earlier practice.

^e In Béarnais *éous* (1 *écu* = ca. 1.35 *livres tournois*, a parity retained throughout most of the period; Charles Dartigue-Peyrou, *La Vicomté de Béarn sous le règne d'Henri d'Albret, 1517-1555* [Paris, 1934], 299-302). The figures are for gross receipts in money; payments in kind have been omitted because they were relatively stable and came to less than 2 percent of the gross receipts. Accounting methods caused the receipts for 1589-90, 1594-95, and 1600 to be inflated.

The viscounty of Villemur was sold to Antoine de La Tour in 1583 for 18,856 *écus*, and in 1593 the baronies of Barbazan and Bégole were surrendered for 30,000 *livres*. Always the right of redemption was retained.²⁹

The year 1585 marked both the close of nearly five years of relative peace and the beginning of a war that lasted until 1598. To prepare for the conflict, Henry alienated more of his domain and indulged in some borrowing to bring the total receipts from his hereditary lands to 628,539 *livres*. Thereafter, the receipts from his domain declined until the end of the war because it was smaller (see Table 3), and he curtailed the number of alienations. But the accounts of the general treasurer do not reflect the true state of income and expenditures during these years. As early as 1594 Henry repaid Lesdiguières, the Protestant leader in the Dauphiné, the sum of 94,500 *livres*, but he made new alienations through 1596 and, to a lesser extent, thereafter. Indeed, the general treasurer's accounts of 1600 and 1605 suggest that a period of refinancing took place—that old debts were paid and parts of the domain redeemed but that money for these purposes was often obtained by incurring new obligations. That there was an overall surplus by 1605 seems clear (see Table 3).³⁰

Only part of a great noble's income came from his domain. Unless he was in disgrace, he held lucrative royal offices and often received pensions and other gifts. These sums usually do not appear in the accounts of the receipts of the domain, but they were an important part of the total income. Henri d'Albret drew 10,000 or more *livres* annually as governor of Guyenne. In addition, he received special gifts from the crown and from the provincial estates and towns in his governorship. Finally, he had the good fortune to marry the king's sister, which was worth 25,000 *livres* a year from the king, and she brought 10,000 more in her own right.³¹

THE EXAMPLE OF BÉARN SUGGESTS that a noble's income from his domain could increase throughout the sixteenth century, unless he alienated some of his lands or his property suffered serious damage from the wars (as was the situation in Armagnac). The critical question, however, is whether a noble's revenue rose as rapidly as the price of goods and services that he bought—that is, whether his real income increased. Prevailing scholarly opinion is that it did not, but in France and elsewhere this opinion has been based primarily on a comparison of noble income and grain prices, a practice that entails serious problems. In the first place, scholars have generally derived their price indices from the Paris grain market. Between the periods 1526–35 and 1578–82, wheat prices at Paris rose at about the same rate as those at Toulouse, but shortly thereafter they increased more sharply (see Table 4). By 1588–92 they were over twice as high in

²⁹ P. Raymond, "Extraits des registres de la Chambre de Comptes de Pau (XVI–XVIII siècles) d'après un manuscrit appartenant au baron de Laussat," *Bulletin de la société des sciences, lettres, et arts de Pau*, 2d ser., 1 (1871–72): 190, 260.

³⁰ Raymond, *Inventaire-sommaire des archives départementales*, B3165, B3194, B3199, B3407; and AD-PA, B168, B171, B175.

³¹ Dartigue-Peyrou, *La Vicomté de Béarn*, 247; and Robert R. Harding, *Anatomy of a Power Elite: The Provincial Governors of Early Modern France* (New Haven, 1978), 135–42.

TABLE 5
Gouberville's Average Annual Expenses, 1549-63

Category	Cost		Percent
	Livres	Sous	
Clothes and Fabrics	51	5	28.3
Fruits and Sugar	0	15	0.4
Meat and Game	19	13	10.9
Fish	1	12	0.9
Spices and Salt	1	2	0.6
Beer for the Harvesters	0	6	0.2
Bread for the Harvesters	0	10	0.3
Candles	3	2	1.7
Legal Expenses	29	18	16.5
Travel Expenses	28	12	15.8
Wages	44	1	24.4
TOTAL	180	16	100.0

SOURCE: Emmanuel Le Roy Ladurie, Introduction to *Un Sire de Gouberville, gentil-homme campagnard au Cotentin de 1553 à 1562*, ed. A. Tollemer (Paris, 1972), xliii.

the capital as in the southern city. Undoubtedly, the difference reflects the consequences of the semi-siege that Paris endured between 1589 and 1593 and of troop movements that impeded grain shipments before and after that period. The use of the Parisian indices for the period roughly from 1585 to 1598, therefore, greatly exaggerates the rate of inflation. Nevertheless, gross receipts from the domain in Béarn increased more rapidly than Paris wheat prices except in the late 1580s and early 1590s, when gross receipts approximately kept pace with grain prices. Receipts from the domain clearly outperformed the Toulouse wheat market, the nearest locality for which grain indices are available. In the second place, prices in general rose more slowly than grain prices. Local wine in Toulouse actually sold for less during the period 1583-87 than it had during 1526-35, and it was only 20 percent higher during the period 1608-12 than it had been during the mid-1580s (see Table 4). Clearly, we must consider what a noble purchased with his income.

The requirements of great nobles, such as the Albrets, differed from those of typical country gentlemen. A gentleman counted himself lucky if he owned a fief or two. He probably produced nearly all of the food his household consumed either by the direct exploitation of all or part of his domain, by the sharecrop system, or by the payment of dues in kind. Guillaume de Murol, an Auvergne noble of the early fifteenth century, for example, produced all or nearly all of the grain he and his household of about twenty consumed. Furthermore, the value of the bread they ate came to only 22.7 percent of their diet, which in turn was only a little more than 60 percent of their expenses in money and in kind. Even if Guillaume had had to purchase his bread, it would have come to less than 14 percent of his total expenses.³² About a century and a half later the Sire

³² Charbonnier, *Guillaume de Murol*, 196-97, 205-06, 217-18, 233-34.

de Gouberville also grew his grain and baked his bread. Only on special occasions did he purchase a higher quality bread from a baker. Bread was an infinitesimal part of his expenses. Over half of his expenditures between 1549 and 1563 were for clothes, fabrics, and wages (see Table 5). Available indices suggest that industrial prices rose less than half as rapidly as grain prices and that the wages of skilled workers rose more slowly still.³³ Gouberville also spent significant amounts for meat and game, travel, and legal expenses, but the prices of such things certainly did not keep pace with grain. Very likely, during most of the sixteenth century, the cost of living index for the typical country gentleman rose about half as fast as grain prices. If this is true, the country gentlemen prospered during most of the sixteenth century.

The expenses of great nobles, such as the Albrets, were more varied. It would be logical to assume that these nobles spent more on food than simple gentlemen because of their peripatetic way of life. Although they probably set aside enough land to support the permanent residents of each of their châteaux, substantial purchases were necessary when the duke or count arrived with his entourage of ladies, gentlemen, and menial servants. The Albrets were often accompanied in their wanderings by several hundred persons. Daily expenses for food, wine, feed for animals, lodging, and the like were recorded, totaled monthly, and entered in the household account as ordinary expenses.³⁴ In peaceful years, these expenses came to almost half of the annual outlay for the household, but efforts to economize reduced the proportion to little more than a third during the troubled war years (see Table 6). Household expenses, however, were but a small part of the Albret outlay, especially during the Wars of Religion; expenses for food were therefore only a minor part of the family's total expenses (see Table 3, above). The cost of most of the goods and services the Albrets purchased lagged far behind food prices. Indeed, their household and monthly expenses rose rapidly until 1565, reflecting the growing affluence of the family, but both dropped sharply by 1569 because of Jeanne's decision to participate in the war. Needless expenses were curtailed; and, by 1574, the household servants who were paid numbered hardly more than a fourth of the total in 1565 (see Table 6).

Even in their economizing periods the Albrets employed a large number of persons with diversified talents, who must have reduced the family's need to

³³ E. H. Phelps Brown and Sheila V. Hopkins, "Wage-Rates and Prices: Evidence for Population Pressure in the Sixteenth Century," *Economica*, n.s., 24 (1957): 289-306. The authors' source for France is not reliable, but their study shows so much correlation between wage and price movements in France, southern England, and Alsace that their estimates for France are probably fairly close to being correct. Using a base of 1451-75 = 100, they found that in 1601-20 the indices are as follows:

<i>Index of</i>	<i>Alsace</i>	<i>Southern England</i>	<i>France</i>
Prices of foodstuffs	517	555	729
Prices of some industrial products	294	265	335
Builders' wage rates	150	200	268

³⁴ Some of these "ordinary expense" accounts survive, but I have not examined them. See, for 1571, AD-PA, B18, B19; for 1572, *ibid.*, B23; for 1576, B26, B27; for 1577, B33; for 1578, B37; for 1579, B42, B43; and so forth.

TABLE 6
Accounts of the Treasurers of the Royal Household

<i>Year^a</i>	<i>Total Payments^b</i>	<i>Ordinary Monthly Expenses^b</i>	<i>Number in Household Paid</i>	<i>AD-PA Source</i>
1556	64,879	30,340	171	B6 ^c
1560	76,242	33,167	116	B8
1565	124,870	56,758	242	B13
1569	92,871	31,522	120 ^d	B15
1574	51,990	28,266	61	B24
1583	59,751	34,429	77	B83
1588	102,643	38,775	75	B129
1596	293,056 ^e	130,739	128	B140

^a The fiscal year was from January 1 to December 31.

^b All payments and expenses are given in *livres tournois*.

^c I have omitted 83,888 *livres* carried over from the preceding account. The jump in costs was probably caused by Catherine's recall from Béarn in October 1592. She was apparently at court in 1596. To meet her expenses, Henry IV gave her 180,000 *livres*, which were added to the household account.

^d Actually, 200 members of the household are listed, but only 120 were paid. A few are listed in other years who were not paid.

^e This account is printed in part in P. Raymond, "Notes extraites des comptes de Jeanne d'Albret et de ses enfants, 1556-1608," *Revue d'Aquitaine et du Languedoc*, 10 (1866): 565-72.

seek the assistance of outside professionals and artisans. Its employees included clergymen, doctors, druggists, tailors, embroiderers, tapestry makers, pastry cooks, and potters as well as numerous ladies and gentlemen in waiting, household servants, cooks, and stable hands. A comparison of the wages paid to various types of employees in 1529, 1565, and 1596 reveals the striking fact that many remained unchanged, and in 1596 only the two *maréchaux des logis* and the two *aides en fourrière* enjoyed salaries that were more than 25 percent higher than those that their counterparts had received in 1529, despite two-thirds of a century of inflation (see Table 7). The 128 paid employees of the household in 1596 received an average annual salary that was only 24.2 percent more than that of the 136 employees in 1529. Part of even this modest increase must be attributed to the fact that a greater proportion of the employees held upper-echelon positions at the end of the century. It was fortunate indeed for the poorer employees that they ate at the Albret table.

Formulating a cost of living index for the Albrets or any other great noble family is difficult. They bought some food, but food was a very small part of their total expenses. They had many servants, but they made no attempt to increase their salaries commensurate with inflation. Their costs rose, but there is no reason to believe that the average price of the things they bought increased more than one-half or, at most, two-thirds as rapidly as grain prices. If this is true, the Albrets would obviously have prospered throughout the century had peace been preserved. From their accounts we see that, as their revenue increased in the peaceful years, they permitted themselves more luxuries and

TABLE 7
Paid Officials in the Households of Marguerite d'Angoulême,
Jeanne d'Albret, and Catherine de Bourbon

Office ^a	1529 ^c		1565 ^d		1596 ^e	
	Number Employed	Average Salary ^b	Number Employed	Average Salary ^b	Number Employed	Average Salary ^b
Maîtres d'Hôtel	2	400	6	300	2	400
Écuyers	8	200	13	200	4	250
Dames & Demoiselles	5	360	9	322	10	360
Femmes de Chambre	5	80	13	80	7	78
Maréchaux des Logis	2	200	4	200	2	300
Fourriers	3	100	5	100	4	120
Contrôleurs	2	150	4	150	4	175
Médecins	2	300	5	400	1	400 ^f
Valets de Chambre	5	110	25	100	10	120
Sommeliers	4	100	8	100	5	120
Aides à Cheval	12	80	23	83	5	80
Aides à Pied	4	25	5	25	7	50
Écuyers de Cuisine	2	110	6	100	2	120
Garde-Vaisselle	1	110	2	125	1	120
Fourrière	2	100	5	100	2	120
Aides en Fourrière	2	25	4	30	2	80
Portiers	2	60	1	20	1	50
Valet des Pages	1	20	1	29	1	20

^a Officials not appearing in all three accounts have been omitted.

^b All salary figure are in *livres tournois*.

^c Abel Lefranc and Jacques Boulenger, eds., *Comptes de Louise de Savoie et de Marguerite d'Angoulême* (Paris, 1905), 67-77, 80-87. Where there is a discrepancy, I have used the first list.

^d P. Raymond, "Notes extraites de comptes Jeanne d'Albret et de ses enfants, 1556-1608," *Revue d'Aquitaine et du Languedoc*, 11 (1867): 119-25.

^e AD-PA, B140.

^f One médecin, who received only 60 *livres*, has been omitted.

greater display. When they entered the Wars of Religion, they reduced needless expenses and devoted their resources to the military effort. When in 1572 it looked as though the marriage between Henry of Navarre and Marguerite of Valois would insure a period of peace, Jeanne spent 32,133 *livres* for jewels and other things for the wedding, although she had to borrow at least part of the money to make these purchases. But jewels were not a useless luxury. They could be transformed into cash in case the war was renewed more quickly than parts of the domain could be alienated.³⁵ She had mortgaged her jewels before, and doubtless her son made good use of them in the years ahead.

³⁵ Raymond, "Notes extraites des comptes de Jeanne d'Albret et de ses enfants, 1556-1608," *Revue d'Aquitaine et du Languedoc*, 11 (1867): 182, 184.

Some may argue that the Albrets' ability to increase their revenues was atypical. It is true that, as sovereigns of Béarn and Navarre, they had the right to coin money and to ask the estates for donations. But they farmed the right to make coins for ever increasing amounts that paralleled the general growth of their revenues, and the donations of the estates of Béarn were not included in the domainal revenue.³⁶ Granted, in Foix and Armagnac, where the estates occasionally voted the Albrets a donation, and in Navarre, where they regularly did so, the sums collected were incorporated into the domainal revenue, but only in these lands did the sources of their income differ significantly from those enjoyed by other great nobles. Others may argue that Henri d'Albret was an exception, because in his later years he resided on his estates and took an interest in making them efficient, and that Jeanne, his daughter, and Catherine, his granddaughter (who served as Henry of Navarre's regent after 1577), were captives of the so-called Protestant ethic. Fiscal responsibility came naturally to them, and as women they had fewer expenses than men. The financial practices of other noble families must therefore be considered.

The finest extant records of any family are those of the La Trémoilles, and these records have been carefully studied by William A. Weary. The La Trémoilles had no more need to wait for the bourgeoisie to invade the countryside and teach them how to administer their estates than did the house of Foix-Navarre-Albret. During the lordship of Georges de La Trémoille, who is best known for his violence and intrigues during the days of Joan of Arc, a detailed memorandum was prepared on how the family's extensive estates should be managed. Special attention was paid to bookkeeping and the preservation of records. A century later, one of Georges's descendants, François de La Trémoille, was an equally vigorous administrator who extracted every possible *sous* from his tenants and even prepared his own legal briefs.³⁷ "Through marriage, purchase, and law-suit" such men as these managed to increase the number of their estates. In 1486, the family owned thirteen; in 1550, they had thirty-five.³⁸

The La Trémoille family usually leased their land for three years, which provided them with ample opportunities to raise rents to accommodate inflation. They also made use of sharecroppers. The results were striking. Their average annual seigneurial income was 8,200 *livres* in 1486–96; by 1525–41 it had risen to 26,200 *livres*. Furthermore, the average salary and gifts the family annually received from the crown increased from 3,000 *livres* during the period 1486–88 to 18,200 *livres* during the period 1508–10. The La Trémoille fortunes occasionally took a turn for the worse, as, for example, when François had to provide for five sons and give dowries to two daughters. But, aided by several profitable marriages and, above all, by efficient administration, the general trend of the family

³⁶ In 1566, the right to coin money was farmed for six years at 5,000 *livres* per year; in 1579, at 5,500 *livres*; and, in 1585, at 7,000 *livres*; J.-Adrien Blanchet, *Histoire monétaire du Béarn* (Paris, 1893), 9–11.

³⁷ Weary, "Royal Policy and Patronage in Renaissance France: The Monarchy and the House of La Trémoille" (Ph.D. dissertation, Yale University, 1972), 33–58, 226–33, and "The House of La Trémoille, Fifteenth through Eighteenth Centuries: Change and Adaptation in a French Noble Family," *Journal of Modern History*, 49 (1977): "demand article."

³⁸ Weary, "Royal Policy and Patronage in Renaissance France," 36–37.

fortune was upward. In 1500, the family's revenues were the equivalent of 220 kilograms of silver; in the 1530s, 600 kilograms. A division of the estate and the Wars of Religion led to a drop to 430 kilograms in 1619, but a recovery followed. In 1679, the family income was the equivalent of 585 kilograms of silver and, in 1709, 710 kilograms.³⁹

THE ALBRETS AND THE LA TRÉMOILLES were among the richest nobles in France. As such, they could and did employ a number of able men to handle their affairs. Thus, great nobles might have prospered even though they had been guilty of all of the follies historians have attributed to them. We must ask, therefore, how well nobles with less far-flung estates and fewer servants coped with inflation and war.

The Gascon baronies of Auzan provided their lords with 15,000 *livres* in 1454 and 86,000 *livres* in 1614.⁴⁰ The domain in the barony of Auneau near Chartres was subdivided into farms and leased to sharecroppers for about 4 *livres*, 8 *sous*, 6 *deniers* per *hectare* in 1551–60. In the seventeenth century it brought 12 *livres* per *hectare*, an increase that did not quite match the rise in grain prices but was more than enough to compensate the barons for the increased cost of the goods and services they actually bought. Furthermore, the domain consisted of only 699 hectares in 1560; a century later, because of the purchase of parcels of land, there were 1,100 *hectares* to support the noble lord.⁴¹ Domainal revenue was, of course, closely related to land rents, and land rents increased about eightfold in the region around Paris between the reign of Francis I and 1670–75, far more than enough to compensate for inflation. Leases that were paid in kind approximately doubled during the sixteenth century. Picardy and the remainder of northern France followed a similar pattern, but rents in kind in Languedoc appear to have been more stable during the sixteenth century.⁴²

In Normandy the demand for land led to a threefold increase in rent for parcels at Déville-lès-Rouen between 1480 and 1510 and from an eight- to tenfold increase at Hautot-sur-Dieppe between 1480 and 1550. The Norman counts of Tancarville rented their marshlands for grazing in 1459 for 300 *livres* per year. In 1498 they received 550 *livres*; in 1515, 900 *livres*; in 1521, 1,450 *livres*. Although leases were for nine or twelve years, the rents from their land grew from 110 *livres* per year in 1506 to 550 *livres* in 1554, after which the first War of Religion likely caused the drop to 493 *livres* in 1563. The total revenue from the county grew less rapidly because seigneurial dues stabilized; nevertheless, it increased

³⁹ *Ibid.*, 82, 205–06, 216, and “The House of La Trémoille.”

⁴⁰ Ducruc, “Du revenu des baronnies d’Auzan depuis le XV^e siècle jusqu’à la fin de l’ancien régime,” *Revue de Gascogne*, 21 (1880): 482.

⁴¹ J.-M. Constant, “Gestion et revenus d’un grand domaine aux XVI^e et XVII^e siècles d’après les comptes de la baronnie d’Auneau,” *Revue d’histoire économique et sociale*, 50 (1972): 165–202.

⁴² J. Jacquart, “La Rente foncière, indice conjoncturel?” *Revue historique*, 253 (1975): 355–76; and B. Veyrasat-Herren and E. Le Roy Ladurie, “La Rente foncière autour de Paris au XVII^e siècle,” *Annales: Économies, sociétés, civilisations*, 23 (1968): 541–55. Income did decline in the Paris region during the Wars of the League because the destruction made it difficult to collect rent.

from about 2,200 *livres* in 1460 to an average of 7,000 *livres* annually between 1540 and 1550, more than enough to compensate for inflation but not sufficient to restore fully the prosperity the counts had enjoyed before the English invasion in 1415.⁴³

Around 1400 the Norman barons of Neubourg were making the most of their resources, but their income toppled following the English conquest in 1418. The yield from their fiefs, which stood at 739 *livres* in 1405–06, fell to 264 *livres* in 1444–45. The expulsion of the English was followed by a slow recovery that was initially made possible by an increased emphasis on livestock. Scanty surviving evidence suggests that rents and other revenues increased rapidly during the sixteenth century. In 1539, land was leased at 4 *livres* per acre, but in 1591 it brought three times that amount.⁴⁴ Here, as in Béarn, a noble's income from his domain could keep pace with inflation even during the Wars of Religion. He had only to refrain from alienating his land and to hope that plundering troops did not despoil his tenants so that they could not pay their rent.

Only with difficulty could an important noble avoid taking part in such a conflict, and, once committed to one side or the other, he was obligated to raise troops and support the cause. Lands had to be alienated and sometimes sold to pay soldiers. Most likely, the more cautious noble profited by the opportunity to obtain the proffered fief, for the peasant and the burgher were rarely in a position to engage in such large transactions.⁴⁵ Initially, a noble governor or commander may have suffered financially; in the long run, however, the crown compensated for most, if not all, of his expenses if he served the royal cause or paid him to desist if he fought under a rebel banner. In all, Henry IV gave at least 24,000,000 *livres* to League leaders in return for their acceptance of him as king.⁴⁶ Provincial estates and towns were more open-handed than usual when they felt threatened or sought a favor. Pillaging could be profitable, and more than one governor or commander helped himself to royal taxes. Matignon was said to have "entered Guyenne with 10,000 *livres* of rent and died having acquired 100,000 *livres* in the twelve years that he was governor."⁴⁷ His successor, Alfonso d'Ornano, a Corsican, had come to France in 1569 to seek his fortune, and he managed to leave his family well established in its new land. Gaspard de Saux-Tavannes reportedly seized 60,000 *livres* in the sack of Mâcon and, more certainly, made the family fortune in Burgundy. Blaise de Monluc slowly increased his wealth prior to the Wars of Religion, but his estate grew rapidly after that conflict began. He left his heirs a fortune sixteen times as large as that of his grandfather. François de Bonne's early career coincided with the opportunities provided by the religious wars. Initially, he received only a few hundred

⁴³ Bois, *Crise du féodalisme*, 324, 320, 228, 231–33.

⁴⁴ Plaisse, *La Baronnie du Neubourg*, 225, 326, 339–43, 372–73, 539, 551.

⁴⁵ I base this statement on a casual inspection of those who acquired the fiefs that the Albrets alienated and on the list of those who engaged in land transactions with the dukes of Nevers; for the latter, see Harding, *Anatomy of a Power Elite: The Provincial Governors*, 228–30.

⁴⁶ J. Russell Major, *Bellière, Sully, and the Assembly of Notables of 1596*, Transactions of the American Philological Society, no. 64, pt. 2 (Philadelphia, 1974), 4.

⁴⁷ Harding, *Anatomy of a Power Elite: The Provincial Governors*, 135.

livres per year in rents; but by 1610 his annual income reached 121,699 *livres*, and he later became Duke of Lesdiguières and the last constable of France.⁴⁸ Great wealth was also amassed by Jean Louis de Nogaret de La Valette, who profited from royal favor to become Duke of Épernon; others similarly chose the court as the route to fortune during the reign of Henry III.

Unlike Monluc and Lesdiguières, the Nevers were very wealthy before the Wars of Religion began. They also committed every folly that historians have long attributed to the nobility as a whole. Expensive entertainment, gambling, large dowries, and conspicuous consumption were coupled with participation in foreign wars when no domestic conflict was available. One member of the family had to pay a ransom of 125,000 *livres*, and another revived the outmoded practice of crusading. Even biology appeared to be their enemy, for the Nevers failed to produce a male heir but had to provide large dowries for their daughters. At times, they could not pay their debts and had to sell extensive estates. Yet, in spite of everything, the family income, which stood at 115,085 *livres* in 1551, reached 446,260 *livres* in 1612. In 1550 the duke was worth about 2,000,000 *livres*, but after the male line became extinct in 1637 the family's immovable property was valued at 8,062,034 *livres*. Even after a debt of 2,500,000 *livres* is subtracted and inflation is taken into account, the family would appear to have more or less held its own.⁴⁹

There were, of course, nobles who were punished for their folly. Louis de Lausignan, for example, built up a fortune through able and loyal service to the crown, but his son, Guy, lost substantial sums gambling, joined the Catholic League, and sought to become a spy for Philip II of Spain. In the end he re-entered royal service, but neither he nor his son enjoyed Louis's success at court; and the family sank back into relative obscurity.⁵⁰

Undoubtedly, a great noble who employed competent officials to manage his affairs could increase his fortune during the first half of the sixteenth century and could, even if the religious wars were costly to him, recoup his fortune after peace was restored.⁵¹ Furthermore, the wars provided an opportunity for governors and troop commanders to make their fortunes, whether they chose the royalist cause or backed the rebels. But what of the typical noble who counted himself lucky if he drew several hundred *livres* from his lands when the Wars of Religion began? Since such nobles numbered in the thousands, to cite a few instances when they bought additional land or had to sell their diminutive estates is insufficient to demonstrate that they were a rising or a falling class. Only a quantitative study can suggest their fate, and such a study exists for the nobles in the *élection* of Bayeux.

⁴⁸ *Ibid.*, 137, 149–52, 157; and Robert Forster, *The House of Saulx-Tavannes* (Baltimore, 1971), 1–5.

⁴⁹ Harding, *Anatomy of a Power Elite: The Provincial Governors*, 143–49; and J. Russell Major, "The Crown and the Aristocracy in Renaissance France," *AHR*, 69 (1963–64): 634. If 1625 is taken as the terminal date, there is no doubt that the family profited; its debt was then only 91,000 *livres*.

⁵⁰ Harding, *Anatomy of a Power Elite: The Provincial Governors*, 154–57.

⁵¹ The fortunes of the peers increased after 1589; and the nobility as a whole prospered in the *élection* of Bayeux between 1639 and 1666, and probably over a longer period. See Jean-Pierre Labatut, *Les Ducs et pairs de France au XVII^e siècle* (Paris, 1972), 248–50; and James B. Wood, *The Nobility of the Élection of Bayeux, 1463–1666* (Princeton, 1980), 126–28.

The most striking thing about the nobles of Bayeux is how few of them had active military careers. Between 1552 and 1639, only 3 to 13 percent of them annually were in regular service. In those rare instances when the *ban* was summoned, the number of noblemen in the army varied from only 13 percent in 1568 to 38 percent in 1597, after the Spanish had taken Amiens. The exceptionally heavy turnout in 1597 resulted from the crown's decision to include in the feudal levy nobles who did not own fiefs and to impose heavy fines on those who failed to appear. Another notable fact is that the wealthier nobles predominated among those who became soldiers. "Almost three-quarters of the *élection*'s military nobility belonged to the richest half of the nobility, 43 percent (169 of 391) to the richest quarter alone."⁵² Thus, poverty did not drive the nobility to arms. Most poor nobles remained at home, where they produced their own food and thereby escaped the worst effects of inflation. There is no reason to believe that their economic position was less favorable at the end of the Wars of Religion than at the beginning, provided that their lands had not been plundered too often.

Arguments that real noble income declined during the last part of the sixteenth century are based largely on evidence that there were many poor nobles at the end of the period and that nonnobles were purchasing fiefs. James B. Wood has demonstrated, however, that in the *élection* of Bayeux the proportion of poor nobles in 1639 was almost identical to what it had been in 1552 and that, although some commoners bought fiefs from nobles, nobles were more active in buying fiefs from commoners.⁵³ As a result, 87 percent of the fiefs in the *élection* of Bayeux belonged to nobles in 1562 on the eve of the Wars of Religion, 88 percent in 1597 at their close, and 93 percent in 1640. Nor did new nobles dispossess the old nobility. Indeed, the old resident nobility held 57 percent of the fiefs in 1552 and 62 percent in 1640, despite the unusually large number of patents of nobility that the crown sold during the Wars of Religion. In the long run, the nobility also increased its share of the revenue from fiefs. In 1552 and again in 1562, 8 percent of the income from fiefs went to commoners and unidentified persons. By 1587 their share had increased to 13 percent, but thereafter a decline set in. In 1597 commoners and unidentified persons received 11 percent of the income; in 1640, only 2 percent.⁵⁴

In the vicinity of Paris, nobles who owned vast estates—such as the Montmorencys—and those who held court positions or received royal pensions managed to retain, or even increase, their holdings after the Hundred Years War. The scant surviving evidence suggests that they also increased their purchasing power but not to the extent that their financial position of the first half of the fourteenth century was fully restored. Lesser royal officials—that is, those in the sovereign courts and in finance—did penetrate the countryside and purchase some of the fiefs of the poorer gentlemen. The merchant, however, rarely partic-

⁵² Wood, *The Nobility of the Élection of Bayeux*, 95, 82–84.

⁵³ *Ibid.*, 120–26. Wood has defined an old noble family as one that had been noble for a century or more.

⁵⁴ *Ibid.*, 145–49. There were 415 fiefs in the sample in 1562, 417 in 1597, and 487 in 1640.

ipated in this process; in other words, there was no "bourgeois conquest of the soil."⁵⁵ At Lyons, where the merchant rather than the royal official predominated, much of the land was owned by the Church, which prevented commoners from acquiring property before 1525. Not until the lands of the constable of Bourbon and, later, some Church property were put on the market did burghers begin to buy seigneuries. Local noblemen, who were neither wealthy nor numerous, were unable to compete.⁵⁶

In regions more removed from the great political and commercial centers, the old nobility probably fared as well as or better than they did around Bayeux. Normandy was perhaps the richest province in France, and the town of Bayeux, which boasted roughly 6,000 inhabitants, was more important than the typical local seat. To top that wealth the crown sold or awarded no less than 74 patents of nobility in the *élection* between 1541 and 1598,⁵⁷ but in spite of this influx the old nobility, and the nobility as a whole, managed to hold its own.

Income from fiefs represents only part of noble revenues. Many nobles also managed to increase their income by the purchase of church and nonnoble lands. In 1569 the nobility bought over one-fourth of the goods sold by the clergy to meet royal demands in the twenty-two dioceses of the *généralités* of Paris, Champagne, Lyons, Berry, and Dijon. Other sales followed in these and other parts of France. In all, between 1568 and 1586 property worth 13,562,000 *livres* was alienated, much of which was not seigneurial.⁵⁸

More significant was the nobles' purchase of peasant land to round out their estates. After the Hundred Years' War the nobility of the Gâtine in Poitou took the lead in buying the scattered parcels of land belonging to peasants in order to form more consolidated farms, which they rented to sharecroppers on five-year leases. In the sixteenth century, nobles usually demanded payment of a specified amount of agricultural produce rather than a fixed percentage of the crop, as later became the custom. This practice favored the nobility, because crop yields fell toward the end of the century, a development that may explain why about 60 percent of the peasants abandoned their leases between 1570 and 1590. In Burgundy, the damage done by the Wars of Religion as well as indebtedness caused many peasants to part with their holdings and the common lands on which so many of them depended. In the Beauce the nobility, both new and old, acquired substantial holdings between 1543 and 1696.⁵⁹

The nobles' purchase of peasant holdings created a serious problem in prov-

⁵⁵ Fourquin, *Les Campagnes de la région parisienne*, 482, 465–82; Jacquart, *La Crise rurale en Île de France*, 66–85, 161–63, 245–47; and Veyrassat-Herren and Le Roy Ladurie, "La rente foncière autour de Paris," 549–55.

⁵⁶ Richard Gascon, *Grand commerce et vie urbaine au XVI^e siècle: Lyon et ses marchands*, 2 (Paris, 1971): 813–30.

⁵⁷ Wood, *The Nobility of the Élection of Bayeux*, 64.

⁵⁸ Le Roy Ladurie, "Les Masses profondes," 699–710. For purchases of Church lands in Languedoc, see his *Les Paysans de Languedoc*, 1: 362–71; and in Périgord, see N. Becquart, "Les Aliénations du temporel ecclésiastique au diocèse de Périgueux de 1563 à 1565," *Annales du Midi*, 86 (1974): 325–33.

⁵⁹ Louis Merle, *Le Métaire et l'évolution agraire de la Gâtine poitevine de la fin du Moyen Âge à la Révolution* (Paris, 1958), 63–66, 91, 178–79; P. de Saint-Jacob, "Mutations économiques et sociales dans les campagnes bourguignonnes à la fin du XVI^e siècle," *Études rurales*, 1 (1961–62): 34–49; and J.-M. Constant, "La Propriété et le problème de la constitution des fermes sur les censives en Beauce aux XVI^e et XVII^e siècles," *Revue historique*, 249 (1973): 373–76.

inces where the *taille* was *personnelle*, because such property was withdrawn from the tax digest and the difference was made up by increasing the burden on the remaining peasant holdings. In Dauphiné an exceptionally bitter dispute arose because the villagers were able to produce a legal argument that the *taille* was *réelle* in that region—that is, that the nobility and other privileged groups should pay taxes on the common land that they owned. During Henry IV's reign, the villagers gathered data to prove that from one-half to two-thirds of the taxable land had passed into the hands of those who claimed to be exempt. Perhaps the peasants' figures should not be fully trusted, but the king's council recognized that they had a legitimate complaint. Finally, in 1634, after the dispute had raged for nearly a century, a decision favorable to the peasants was decreed.⁶⁰ In 1594 the third estate of Haute-Auvergne complained that the nobility had bought nearly half of the land owned by poor farmers. Thirteen years later the third estate of Forez and Beaujolais and the *plat pays* of Lyonnais asked the king to forbid the nobles to acquire rural property outside the jurisdiction of their justice and to make them either pay the *taille* on or surrender the lands that they had already bought. The king's council thought that the complaint was serious enough to warrant asking royal officials in the provinces to give their opinions on the request.⁶¹

As might be anticipated in provinces where the *taille* was *réelle*, the privileged sometimes sought to have it declared *personnelle* so that they would not be taxed on the nonnoble land they had acquired. The effort to redefine the *taille* surfaced in Agenais, Condomois, Provence, Languedoc, and probably elsewhere; the movement was most often spearheaded by royal officials and university faculties. In general, the effort failed because of the strength of tradition and because of the crown's realization that reducing the number of taxable acres was not in its interest.⁶² The nobility, to its credit, did not press the issue vigorously, although by the eighteenth century nobles owned over half of the taxable land in parts of Languedoc. That the purchase of peasant holdings had become a national problem was brought forth during the Assembly of Notables of 1626–27, but a move to declare the *taille* to be *réelle* in all of France came to naught because those who attended were all among the privileged.⁶³

THIS ESSAY HAS FOCUSED on the income nobles derived from land because land provided most of their sustenance. But, of course, this is only part of the picture. Many nobles profited greatly from the crown and the church and from invest-

⁶⁰ Major, *Representative Government in Early Modern France*, 76–80, 232–35, 326–32, 433–36, 481, 524–26, 531–32, 597, 614–16.

⁶¹ Gabriel Esquer, ed., *Inventaire des archives communales de la ville d'Aurillac antérieures à 1790*, 1 (Aurillac, 1906): AA21; and Noël Valois, ed., *Inventaire des arrêts du Conseil d'État, règne de Henri IV*, 2 (Paris, 1893): nos. 11,232, 11,746.

⁶² Major, *Representative Government in Early Modern France*, 65, 277–78; and Richard J. Bonney, *Political Change in France under Richelieu and Mazarin, 1624–1661* (Oxford, 1978), 365.

⁶³ Major, *Representative Government in Early Modern France*, 65, 503. Among the few who supported the proposal in the Assembly of Notables was Louis de Marillac, the brother of the keeper of the seals.

ments in mining, glass making, *rentes*, and trade. Some engaged in usury; of the 79,949 *écus* nine villages in the Rhône valley borrowed during the Wars of Religion, 39,040 *écus* came from the nobility, 31,856 from the bourgeoisie, 4,026 from the military, and 5,037 from persons of unknown origin.⁶⁴ Moreover, insufficient attention has been given to the economic problems posed by overly large progenies. With these limitations in mind and on the basis of the data now available, the following conclusions are justified.

1. The improved methods in accounting and exploiting the soil must be attributed to the nobility and its employees who adopted the new procedures before there was a significant bourgeois penetration of the countryside.
2. The income from the typical fief increased as rapidly as or more rapidly than grain prices until the Wars of Religion, because the nobility used short-term leases, usually of three to six years, that quickly responded to the growing peasant demand for land that was sparked by a demographic explosion.
3. The price of the goods and services that nobles purchased rose at about half the rate of grain prices, or possibly at two-thirds that rate in the case of the great nobles. As a result, the landowning nobility became more affluent. In all likelihood, its purchasing power more than doubled prior to the Wars of Religion. Therefore, insofar as nobles as a class had an economic motive for participating in the religious wars, it was to increase their already growing affluence. Truly poor nobles rarely took part. Even if they had desired to do so, they would have had difficulty buying the necessary equipment.
4. The domain continued to yield increased income during the Wars of Religion provided that the owner alienated little, if any, part of it and that it suffered little, if any, direct damage from the conflict.
5. Most important nobles who participated in the war had to borrow money and alienate part of their estates to pay their soldiers. Other nobles suffered from the depredation of troops and the loss of livestock that provided fertilizer for their land. Productivity declined and revenue temporarily failed to keep pace with inflation. The deleterious effects of the wars, which perhaps appeared as early as 1567 and became especially pronounced between 1585 and 1595, largely account for the contemporary literary evidence that the nobility was in economic decline near the end of the sixteenth century. There is no reason to believe, however, that the nobility lost its relative position, because merchants and government officials suffered as much and because some nobles managed to take advantage of the Wars of Religion to make their fortunes.

⁶⁴ Daniel Hickey, "Une Remise en question: Procès des tailles et blocage social dans le Dauphiné du XVI^e siècle," *Cahiers d'histoire*, 23 (1978): 33.

6. The nobles recovered their economic position much more rapidly after the Wars of Religion than they did following the Hundred Years' War. Henry IV paid all or most of their debts by compensating friend and foe alike. The population was larger, and this assured low wages, a strong peasant demand for land to lease, and high prices for agricultural products, although the wartime peak was not maintained. Because of their purchase of peasant and, to a lesser extent, Church lands, the nobles' estates were more extensive than before.

La Noue's observation—"France is so populous and so fertile that what the war has damaged in one year is restored in two"⁶⁵—is an exaggeration. Two years was enough time for the peasant to rebuild his cottage, replace his tools, and plant his fields, but it was not sufficient to rebuild the herds of livestock and to restore the fertility of the soil. This took place more slowly. Nevertheless, noblemen who owned a fief or two as well as the dukes and counts who held many were able to resume a march toward greater affluence, a process that had been only temporarily interrupted by the Wars of Religion.

⁶⁵ As quoted in Jacquart, *La Crise rurale en Île de France*, 254 n. 1.

“A Situation of Inferiority”: French Military Reorganization after the Defeat of 1870

ALLAN MITCHELL

PRUSSIA'S QUICK AND DECISIVE MILITARY VICTORIES in 1866 and 1870 had a profound effect upon Europe. Not only did they bring about the unification of Germany, the reorganization of the Habsburg Empire, and the overthrow of Bonapartist France, but they also, in their astonishing demonstration of Prussian military supremacy, raised questions everywhere about the need for educational and military reforms. Behind every German soldier stood a German schoolmaster—so, at least, went the durable cliché. Both in Britain and on the Continent, Germany was widely admired for its public school and general staff. In educational and military matters, modernity acquired a distinctly teutonic aspect, and Germany was generally regarded as the tutor of Europe. Reformers therefore had to ask themselves two questions. Why was the German population better schooled and better drilled? How far should other nations go in copying the Germans in order to compete with them?¹

In the wake of war, France was most profoundly influenced. Not only defeated but humiliated as well, the French suffered a national crisis that was intellectual and moral as well as military.² German superiority weighed heavily on them in specifically discomfiting ways: months of military occupation, a huge reparations settlement, direct political pressure, interference in religious affairs, intense commercial competition, and more.³ Beyond all of that, the French were obsessed by an elemental and obvious military fact of life: if they were ever again to field an army in combat, risk the national honor, and recover the lost provinces of Alsace and Lorraine, Germany must be overcome. Accordingly, all of French military planning after 1870 was predicated on competition with the

¹ According to Michael E. Howard, “France after her defeat set herself to copy the institutions of the victor as closely as possible, and the remaining powers of Europe followed to a greater or lesser degree”; Howard, “The Armed Forces,” in F. H. Hinsley, ed., *The New Cambridge Modern History*, vol. 11: *Material Progress and World-Wide Problems* (Cambridge, 1967), 211. Britain alone among the major Western European nations declined to adopt a system of universal conscription similar to that of Germany; but the British did join the others in introducing compulsory elementary education.

² See Claude Digeon, *La Crise allemande de la pensée française (1870–1914)* (Paris, 1959), 48–112 *passim*.

³ See Allan Mitchell, *The German Influence in France after 1870: The Formation of the French Republic* (Chapel Hill, 1979). This book is the first of a planned trilogy. Some of the findings of the sequel volume are reported in the present article, which contains only a fraction of the documentation gathered from the military archives at Vincennes.

Bismarckian Reich. France had no other criterion of efficacy and no other measure of potential. If reforms or improvements did not prepare France to emerge victorious from a future combat with the only foe that mattered, they would be of little account. Everything faced eastward.

Historians—most recently George F. Kennan—have long asserted that French military reorganization between 1870 and 1890 was a notable success.⁴ By taking to heart and mind the lessons of defeat, by sustained planning, and by resolving satisfactorily the inherent conflict between civilian and military control, it is alleged, the French laid a solid basis for a vast defensive effort that saved their nation on the Marne in 1914.⁵ Edifying as this version may seem, it is largely apocryphal—a myth perpetuated by historians who have tended to reason backward from an inspiring conclusion and who have relied mostly on the public record. The French military archives at Vincennes contain a different and altogether disquieting story—a tale of indecision, blundering, deliberate falsification, and colossal waste of resources in the two decades after the defeat of 1870.

An obvious example is the problem France faced in recruiting, training, and maintaining its army. The military reform of 1872 was a bad compromise between those who wanted to retain the old professional organization and those who wished to institute universal conscription. Unable to decide between the traditional requirement of seven-year service and the German system of three-year service, they settled for five. But this solution made little sense. Each year about three hundred thousand men reached draft age, of whom nearly one-half were declared too sickly or too short to serve. Since the army could only absorb about one hundred and thirty thousand draftees annually under a five-year system, the reform legislation of 1872 kept the door open for the old abuses of wealth and privilege in evading conscription.⁶ More of a curse than a compromise, this measure saddled the French army with a mode of recruitment that contradicted the simultaneously acclaimed principle of universal military conscription.⁷ Until 1889, one attempt after another to introduce three-year service

⁴ In speaking of "the greatly improved strength and readiness of the French armed forces" by the end of the 1880s, Kennan has concluded that they were "moderately larger than the German ones." Although he noted that his calculations were "distilled from approximately twelve different sources," he did not identify them. Presumably, however, the documentation located at Vincennes was not among the dozen. See Kennan, *The Decline of Bismarck's European Order: Franco-Russian Relations, 1875-1890* (Princeton, 1979), 247, 415.

⁵ One writer has contended, for example, that by the mid-1890s "the military establishment was all that twenty-five years of labor, of intellectual, financial, and concrete effort could make it. . . . The military organization with which France thus equipped herself after the defeat of 1870 was to stand stoutly until 1914. . . . Thanks to the solidity of the Army's over-all organization and the nature of its mission, the military potential of France was no longer dependent upon political vicissitudes." He therefore referred to the period before the Dreyfus Affair as "the golden age" of military reform. Paul-Marie de la Gorce, *The French Army: A Military-Political History*, trans. Kenneth Douglas (New York, 1963), 6-9. David B. Ralston has also made repeated references to "the French military renaissance" after 1870, and he concluded that "the ultimate triumph . . . was a tribute to the soundness and the efficacy of the military institutions of France under the Third Republic"; Ralston, *The Army of the Republic: The Place of the Military in the Political Evolution of France, 1871-1914* (Cambridge, Mass., 1967). A more skeptical and, in my judgment, a far more appropriate tone is evident in the remarkable, much earlier analysis of Joseph Monteilhet; see his *Les Institutions militaires de la France (1814-1924)* (Paris, 1926).

⁶ For an expert analysis of the problem of recruitment, see Bernard Schnapper, *Le Remplacement militaire en France: Quelques aspects politiques, économiques et sociaux du recrutement au XIX^e siècle* (Paris, 1968).

⁷ Hence, it seems unduly sanguine to praise the 1872 law as "a definite step towards the achievement of

was beaten back. By 1890 France thus had, on the one hand, a large and expensive semi-professional standing army and, on the other, an undermanned and insufficiently trained reserve force. As military experts conceded in the secrecy of the Conseil Supérieur de la Guerre, the reform bill of 1872 had given France the worst, not the best, of both worlds. When the French government finally implemented three-year military service in the 1890s, France's rate of population growth had fallen far behind that of Germany, making a French army equal in size to the German army difficult, if not impossible, to achieve.⁸

APART FROM THE PROBLEMS ASSOCIATED WITH RECRUITMENT, French preparedness was lamentably weak in five critical areas during the two decades following the wars of 1866 and 1870: fortifications, artillery, cavalry, infantry, and general staff. These weaknesses, in turn, were compounded by fundamental inadequacies in physical support, training, and leadership at all levels.

According to General Séré de Rivières, the chief architect of France's defensive system after 1870, a principal military deficiency had been the lack of a fixed central point, and the first priority therefore had to be construction of a defensive "screen" (*rideau*) along the Moselle and Meuse rivers. By building an impregnable barrier between the fortresses of Verdun and Toul, Séré argued, the French could divide invading German armies and force them into "corridors" to the north and south, and mobile French forces could then be deployed in accordance with the relative weight of the German thrusts. This plan would provide, as Séré wrote, "an active defense based on several strong points."⁹

Certain corollaries followed from these axioms. In a war of movement, rail transportation would be of utmost importance. The French needed to extend their rail system and, at the same time, build a series of redoubts (*forts d'arrêt*) capable of interdicting enemy traffic along those lines. The construction of an interior defensive line—romantically identified as the *falaise de Champagne*—was also desirable since, in the case of initial setbacks, French armies might need to regroup at some distance from the frontier without retreating all the way to Paris. While urgent priority would necessarily be given to the eastern front from Verdun to Belfort, the French could not neglect their northern border, even though it was considered by most military planners, including Séré, to be "sufficiently protected, at this moment, by Belgian neutrality."¹⁰

the nation in arms"; Richard D. Challener, *The French Theory of the Nation in Arms* (2d ed., New York, 1965), 42. The law is more aptly characterized as "the mutilation of universal conscription"; Monteilhet, *Les Institutions militaires de la France (1814-1924)*, 125.

⁸ By 1911, France was obliged to draft nearly 83 percent of its annual contingent of available manpower, whereas Germany needed to induct only 53 percent. See Alfred Vagts, *A History of Militarism* (2d ed., New York, 1959), 217.

⁹ "Réorganisation des frontières entre la Mer du Nord et la Méditerranée: Rapport de la sous-commission de défense," July 1873, Archives de Guerre [hereafter, AG], Vincennes, MR 2146. The president of this subcommittee was Marshal Canrobert, but Séré was the principal author of its report, which he also countersigned.

¹⁰ Séré de Rivières, "Considérations sur la réconstitution de la frontière de l'Est," November 15, 1873, AG Vincennes, MR 2150, "Exposé du système défensif de la France," May 20, 1874, *ibid.*, MR 2136, and "Organisation du nouveau système défensif," November 10, 1875, *ibid.*, 7 N 1787. Authorship is uncertain of another

With the notable exception of the system of fortifications at Verdun, the defensive scheme proposed by Séré de Rivières remained unrealized—beset as it was by prohibitive expense, perpetual confusion, and advancing technology. The problem of its cost had already become apparent in the 1870s, and a comparison of annual French and German budgets clearly reveals France's dilemma. According to officially published estimates, by 1875 each nation allotted about one-quarter of its national budget—roughly 500 million francs—to its ministry of war.¹¹ But that formal equality is deceiving. The defeat of 1870 left the French with a shattered army bereft of weapons, equipment, and morale. With Alsace and Lorraine they lost their entire eastern defensive system. Thus, while the French had to spend millions for new fortresses, weapons, and equipment, the Germans—for whom defense was in any event less crucial—only had to convert the newly acquired bastions of Metz and Strasbourg in order to face them westward. Confidential documents from the Rue St. Dominique reveal, moreover, that the French calculated military expenditures in ways altogether different from the Germans: a comparative budgetary analysis in 1892 disclosed that the annual German appropriation for the army was equivalent to 730 million francs, whereas the adjusted French figure was only 560 million. The gap between French and German military expenditures widened, rather than narrowed, in the last third of the nineteenth century.¹²

Perpetual confusion resulted from an underlying disagreement among French staff officers as to defensive strategy. By no means all of them accepted Séré's conception of "corridors." His detractors lobbied for construction of more *forts d'arrêt* and smaller detached forts to fill in gaps between the major strongholds. Séré successfully resisted this pressure until about 1877, when he began to lose one vote after another in the closed sessions of the Conseil Supérieur de la Guerre. Unknown to the French public then—and to historians since, who have relied solely on published documents—a substantially different defensive scheme began to emerge by 1880, one that projected compact forts on the French frontiers at every conceivable location. Séré's urgent warnings about the folly of erecting "a kind of Wall of China" were disregarded, and the building of numerous, small, frontier forts proceeded.¹³

contemporary staff report that contains similar arguments; "Note sur le rassemblement d'une armée sur la frontière du Nord dans le cas de la violation de la neutralité de la Belgique par l'Allemagne," February 8, 1876, *ibid.*, 7 N 1812.

¹¹ "Tableau général du budget," 1873–74, Bibliothèque Nationale, Paris, NAF 20641.

¹² "Comparaison des budgets de la guerre en France et en Allemagne," December 1892, AG Vincennes, 7 N 665. This analysis directly contradicts Ralston's assumption that, "for the thirty year period between 1871 and 1900, the tables of comparison show a remarkable similarity between the French and German armies. Both countries spent about the same amount of money on their military establishments during these three decades." *The Army of the Republic*, 132–33. It is, however, a common error. Theodore Ropp, for example, has commented, "In the 1890s France and Germany were spending about the same amounts on their peacetime armies, each of which then numbered somewhat less than half a million men"; Ropp, *War in the Modern World* (Durham, N.C., 1959), 180.

¹³ Commission des places fortes, "Procès-verbal," March 15, 1877, AG Vincennes, 7 N 1803. For a summary of this controversy in a later report, prepared by the Third Bureau of the Ministry of War, see "Note faisant ressortir les opinions émises au sujet de la création et du développement des places d'Epinal, Toul et Verdun dans le sein de la Sous-Commission de Défense présidée par le Maréchal Canrobert, du Comité de Défense, et du Conseil Supérieur de la Guerre," December 1893, *ibid.*, 7 N 1826.

Such a policy might have made more sense had not advancing technology rendered most new French construction obsolete before it was completed. Some of this technological progress was thoroughly predictable, some not. The alarming success of Germany's long-range artillery in 1870 had already betrayed the vulnerability of all but the stoutest fortifications. That awesome force was soon compounded, as the French already suspected, by the introduction of a new generation of breech-loading, rapid-fire steel cannons produced by Krupp. Worse was to come. By the early 1880s there was no longer any doubt that both France and Germany would achieve a scientific breakthrough in munitions. The key was *mélinite*, a chemical substance that greatly enhanced the force of explosives. During the late summer of 1886 the French conducted a series of tests on the fort at Malmaison. Typical of construction of the 1870s, Malmaison's ramparts were made of "lightly sanded" earth reinforced by stonework. A team of military inspectors stood by and watched with horror as the new artillery shells smashed the fortress with "astounding violence." The official report to the Ministry of War concluded that, "in a fortification subjected to such bombardment, any activity would be impossible and, furthermore, that the morale of the garrison would be completely destroyed within a few hours."¹⁴ A later analysis drew the unimpeachable conclusion "that it was necessary to proceed to a complete rearrangement of all of the structures in our defensive system in order to put them in a condition to resist, even for a rather limited time," artillery fire of this sort.¹⁵

The consequences were manifold and depressing. To be truly defensible, a fort required extensive steel and concrete reinforcements. For budgetary reasons, such alterations were possible only at the four major positions on the eastern frontier—Verdun, Toul, Epinal, and Belfort. Of these, only Verdun was adequately "modernized." At Epinal, for example, only 7.5 of 43 kilometers on the defensive perimeter had been reinforced by 1914.¹⁶ Many other forts had to be reclassified as temporary depots or abandoned altogether: no more *forts d'arrêt*, no more ramparts to plug the gaps, no more *falaise de Champagne*. The irony is that, after 1890, the French began to dismantle forts precisely in that region through which a German invasion was increasingly considered likely. Indeed, the French wanted the Germans to attack between Verdun and the Belgian border, or further north if they chose, since the French assumed that there the invading army could most readily be contained and countered. Of necessity, France's defensive strategy was not to block the northern route—that was both financially and technologically infeasible—but to rush mobile forces forward to blunt a German assault. After years of dithering, French planning came full circle: just as Séré had once imagined, the Germans were to be lured into an in-

¹⁴ "Rapport sommaire sur les expériences faites du 11 août au 22 septembre 1886 au fort de Malmaison," September 22, 1886, AG Vincennes, 7 N 46.

¹⁵ "Rapport aux comités techniques de l'artillerie et du génie," March 1, 1909, AG Vincennes, 7 N 11. Also see Philippe Truttmann, "La fortification en VI^e région militaire," *Revue historique de l'armée*, 29 (1973): 55–79.

¹⁶ Jacques Grasser, "Un Exemple de fortifications dites 'Séré de Rivières': Le Camp retranché d'Epinal entre 1871 et 1914," *Revue historique de l'armée*, 29 (1973): 116–34.

visible trap.¹⁷ Thus, the French wittingly did their part to tempt the German army to invade through Belgium.

Not only did the French have to rethink and rebuild their fortifications after 1870, but they also had to re-equip their artillery. Most of their heavy cannons had been destroyed or captured in the conflict, and what remained was admittedly inferior "in range and in accuracy, relative to . . . the Prussian weapons."¹⁸ A new generation of artillery pieces was urgently needed. Since the artillery was the *enfant gâté* of the French army, in August 1873 the Commission Supérieure de l'Artillerie, convened and chaired by Marshal MacMahon himself, decided on 144 new batteries of bronze Reffye 75 and 85 millimeter cannons, despite their staggering cost and the likelihood that they would be outmoded in a relatively short time by the development of steel weaponry.¹⁹ But complicating this decision was French insistence on parity with Germany. Just a few months later, when France learned that the German counterparts to the French field pieces would be 78.5 and 88 millimeters, the then chief of French artillery, General de Rochebouët, claimed that "nearly all of the infantry generals" believed "that we cannot have a caliber inferior to that of the Germans." He therefore proposed that "the caliber of 75 millimeters should not be accepted for . . . the new mobile artillery."²⁰ His successor in early 1874, General Canu, agreed. "Since Prussia has adopted the calibers of 78.5 and 88 millimeters, it currently seems impossible to retain those of 75 and 85. . . . To obtain superiority over the German artillery . . . the Comité [de l'Artillerie] proposes to adopt the calibers of 80 and 90 millimeters." It followed that the costly Reffye system could only be a stopgap until another model—the Bange 80 and 90 millimeter cannons—was adopted in 1877.²¹

Throughout the last three decades of the nineteenth century, a rather literal-minded attempt to keep up with the Hohenzollerns characterized the French attitude, and the pressure of German competition was both unrelenting and disrupting. Whether in fortifications, heavy armaments, or hand weapons, the French needed to do more and spend more if they hoped to challenge their imposing neighbors. In 1891 Charles de Freycinet, then the minister of war, confided to General de Miribel that Germany's "considerable augmentations" had placed France "in a situation of inferiority too dangerous to accept with resignation." And, he continued, "We cannot remain faced with such an increase of forces without taking similar measures."²² A detailed comparative study of mili-

¹⁷ Third Bureau, "Note relative au cas de violation par l'Allemagne de la neutralité belge," October 19, 1888, AG Vincennes, 7 N 1812. The majority view before 1900, for which ample documentation may be found in the archives at Vincennes, was most succinctly expressed by General de Miribel: "La place de Verdun empêche de tourner notre aile droite, l'ennemi essiera donc probablement de tourner notre aile gauche, mais plus il prononcera ce mouvement, plus il tombera dans le piège que nous lui avons tendu." Miribel, "Note sur le travail de Grouard," January 1890, *ibid.*

¹⁸ Lt.-Colonel Allan, "Effets du tir de l'artillerie contre l'enceinte et les forts de Paris," August 21, 1871, AG Vincennes, 9 N 4.

¹⁹ Captain H. Langlois, *Les Artilleries de campagne de l'Europe en 1874* (Brussels, 1875), 8–16. Also see General J. Challeat, *Histoire technique de l'artillerie de terre en France pendant un siècle (1816–1919)* (Paris, 1933), 272–78; and General Thoumas, *Les Transformations de l'armée française*, 2 vols. (Paris, 1887), 2: 135–37.

²⁰ Rochebouët, as quoted in Challeat, *Histoire technique de l'artillerie*, 290.

²¹ Canu, as quoted in Challeat, *Histoire technique de l'artillerie*, 305–11.

²² Freycinet to Miribel, February 24, 1891, AG Vincennes, 5 N 2.

tary expenditures between 1890 and 1914 does not exist, but the best available estimate is that, during that period, special German appropriations for heavy equipment such as artillery was double that of the French. No wonder that in the summer of 1914 a proposal was before the French Assembly for an extraordinary allocation of one billion francs for the purpose of reducing the German military preponderance.²³

THE MOST SERIOUS PROBLEM FOR THE FRENCH ARTILLERY, as it was for other branches of the army, proved not to be design and manufacture of weapons or adequate fortifications but the training and deployment of personnel. Staffing needs were chronic. In 1873, the estimated deficit of trained artillery officers exceeded one thousand; and, because of military expansion in the late 1870s and the 1880s, the number grew rather than diminished. The new forts under construction required more experienced cannoneers, and so did the new mobile artillery field pieces. Consequently, the Comité de l'Artillerie flirted for years with the radical solution of a *dédoublement*: the same cadres of staff officers and non-commissioned officers were to command both active and reserve troops. Much debated and resisted at first, this measure was finally adopted in 1880. Scarcely a year later, however, the presiding officer of the Comité de l'Artillerie, General Berkheim, conceded that "certain complications" were already evident. In no other branch of French military service and in no foreign army, he wrote, was it expected that two units would be commanded and supplied as one. *Dédoublement* resulted in insolidity such that there was "reason to organize the field artillery on a new basis."²⁴ Although subsequently attempted, this reorganization was only piece-meal and did not solve the manpower—or, rather, the trained manpower—shortage, which was aggravated by the large numbers of troops stationed at fortifications that might or might not be attacked during a German invasion.

Despite the obvious problems attached to a system of *dédoublement* for the artillery, which were evident as early as 1881, the infantry adopted a similar system in 1889. As the policy applied to the infantry, the territorial reserves were to be amalgamated in wartime with the active army and its regular reserves. Three nagging deficiencies—along with a host of others—made *dédoublement* seem a desirable option: insufficient NCOs, incompetent reserves, and inadequate training.

In some respects the paucity of noncommissioned officers was potentially the most serious; without them, sufficiently trained manpower in the lower ranks of the infantry was impossible to achieve, even if the infantry were at full strength. Germany seemed to possess a social caste of professional NCOs of a kind that the French, try as they did with benefits and bounties, could neither attract nor retain. A French expert, General Farre, estimated in 1882 that, whereas 90 percent of Germany's NCOs joined their infantry cadres through re-enlistment, the

²³ See Ralston, *The Army of the Republic*, 134–35.

²⁴ "Extrait du Registre des délibérations du Comité de l'Artillerie," January 4, 1882, AG Vincennes, 9 N 40.

comparable figure for France was an incredibly meager 13 percent. "These results," Farre commented, "make the head spin."²⁵

The inferiority of the French infantry in training and tactics was particularly notable in comparison with German procedures. Prussia had a tradition of military maneuvers, war games, and field exercises that was the envy of every army in Europe. Until the end of the 1870s the French organized nothing similar. They specialized instead in parades. It was, as one critic remarked, as if the supreme objective of military training were to cavort on the Champs de Mars. To be sure, maneuvers were expensive, time-consuming, and often frustrating; and, true, replicating the actual conditions of battle was impossible. But, without the training procedures of the German infantry, the French could not hope to form an army of equal competence and self-confidence. Thus, a parliamentary commission formed to study reform legislation could only lament the exaggerated optimism of claiming that France was gradually reaching parity with Germany. Indeed, "the deficit of trained men" had become "a truly enormous figure." Many reserves and territorial troops were receiving only "rudimentary instruction," which was "absolutely insufficient" to fit them for combat. "That is the brutal reality," so concluded the commission's report, "which is aggravated by comparison with the resources that our neighbors have been able to assure themselves, whereas we lack the means to do as they."²⁶

The glaring weakness in the training of infantry, especially the territorial armies, to which the commission referred in 1882, was matched by the lack of reserve strength. France was unable to develop a solid and extensive reserve system, and the obvious disarray of territorial armies was the utter despair of every military inspector who witnessed their performance. A gloomy discussion in the Conseil Supérieur de la Guerre in 1883 centered on dismaying projections that Germany's fully mobilized infantry force in wartime might reach 838 battalions against only 706 for France, a deficit of 132 entire battalions. In part, the problem was simply to raise the number of territorial reserves: "if we do not proceed to [create] new formations," warned General Thibaudin, "we will necessarily be inferior to the Germans." But other military leaders were no less concerned about the quality of such units: "the officers of the reserve and territorial armies," observed General Greseley, "are worth nothing, absolutely nothing." Among the more than a dozen general staff officers who were present and heard this remark, there was not a word of dissent.²⁷

Given such egregious problems, *dédoublement* for the infantry, as it had been for the artillery a decade earlier, was a temporary measure, at best. An inspection team in 1890 reported that the attempted integration of the territorial troops with the regulars would only bring "disaster." The infantry as a whole was, moreover, the "least prepared" of France's combat services.²⁸ By the 1890s the French had developed annual maneuvers much in the German fashion, yet they

²⁵ Farre, *Observations sur les réformes militaires à l'étude* (Paris, 1882), 18–20.

²⁶ Commission temporaire de revision des lois militaires, 2ème sous-commission, "Rapport spécial sur la revision de l'article 34 (formation de guerre de l'armée territoriale)," February 20, 1882, AG Vincennes, 5 N 1.

²⁷ Conseil Supérieur de la Guerre, *Registre des délibérations*, April 7–23, 1883, AG Vincennes, 1 N 3.

²⁸ General de Negrier to Freycinet, May 1890, AG Vincennes, 7 N 44.

never achieved the scale or the sophistication of their mentors. Thus, in the infantry as elsewhere, the pressure of competition with Germany remained as merciless as ever.

Lack of trained manpower was equally critical for the cavalry. The loss of prestige suffered by the entire French army in the war of 1870 had affected no branch of military service more profoundly. The rousing charges of mounted troops across open terrain, so often effective in European conflicts and colonial wars of the past, had only brought disaster when opposed by determined Prussian infantry and artillery. In one skirmish after another, the *furia francese* on horseback was expended to little advantage, and within a few weeks the French cavalry, for all of its ancient and proud traditions, had been routed. Thoroughly humiliated, the cavalry could not hope to retain its former prominence in the postwar years. It needed not only to be rebuilt but reconceived; for, as one of its critics wrote, "the infantry is the basis of the army," and all of the "other branches are only its auxiliaries." But this sensible conclusion came hard to those who had devoted their professional careers to equitation, who relished the panache and splendor of it all, and who foresaw a greater role for mounted firepower on the vast battlefields of the future.²⁹

Although the French attempted to adopt German cavalry organization and tactics, the cavalry continued to suffer from the uneven ability of its officers, the poor conditioning of its steeds, and an overall lack of stamina. After witnessing the first postwar field exercises of mounted troops in 1874, a British observer reported that "several methods have been attempted in an effort to give the French cavalry the qualities of the German cavalry, but these methods have generally failed."³⁰ Just the numbers alone were discouraging. France had entered the war of 1870 with 63 regiments of cavalry composed of 348 squadrons. Combat left ranks decimated and stables vacant. By 1875, after much exertion, the French could boast of 78 regiments; but of these 406 squadrons, 40 were stationed in Algeria and 70 were consigned to interior depots. Hence, the available combat force of French cavalry was actually 296 squadrons of 120 horses each, a total of 35,800 horses.³¹

The infamous horse embargo that Germany imposed on France in 1875 further magnified the cavalry's problems of training and manpower. In terms of the French objective of parity with Germany, France faced a deficit of some twenty thousand horses. Thus, the German embargo, far from being only a diplomatic charade, meant that the French army suffered, since France often had to obtain its animals with great difficulty from Eastern Europe. Under such circumstances, French rearmament as a whole could be set back several years, given the army's dependence on horses for all sorts of functions. A French staff report in 1879 confirmed the deleterious effects of the embargo, warning that Germany maintained a "crushing superiority"—not only in numbers but also in

²⁹ Lt.-Colonel Warnet, Note, August 1, 1873, AG Vincennes, 7 N 45. See Michael Howard, *War in European History* (London, 1976), 103–05.

³⁰ "Les Manœuvres de la cavalerie française: Lettre du correspondant militaire anglais," September 19, 1874, AG Vincennes, 7 N 657.

³¹ Thoumas, *Les Transformations de l'armée française*, 1: 70–73.

quality. If the French ever intended to measure their military strength against that of Germany, the report admonished, they must "immediately effect a radical reform in the acquisition of horses for the army." Otherwise, its superior cavalry would give Germany an important, perhaps decisive, advantage in a future conflict.³²

But such radical reform never came. Instead, available horses within France were often shipped to Germany, either directly via Nancy or through Belgium, rather than sold to the French cavalry. Customs records from the early 1880s show that private traders in France began to acquire horses in large quantity, and by 1885 these transfers to Germany had attained disturbing proportions. Fortnightly shipments of three hundred or more horses were recorded by border officials: an annual rate of at least eight thousand. One agent, the horse-trading firm of Kahn in Paris, was responsible for shipping abroad as many as one thousand animals in a single month. A summary prepared for the French Ministry of War in 1888 calculated that in the previous five years some seventy thousand horses had been dispatched by private merchants to Germany.³³ Manifestly, the Germans were skimming off France's equestrian surplus and constantly adding it to already well-stocked pastures east of the Rhine. This was among the reasons for "the serious crisis through which the French cavalry is passing and which requires a solution"³⁴—words written in 1887 by the same officer, General Grandin, who had chaired the original military commission convened in 1872 to guide the reorganization of the French cavalry. The effort of the intervening fifteen years, for the cavalry as for the artillery and infantry, may not have been entirely in vain. But neither had it brought the French republic markedly closer to military parity with the German Reich.

ALTHOUGH THE FRENCH ARMY after 1870 was designed in so many details to mirror the German, it conspicuously differed in one essential regard—central leadership and command. The *État-Major*, unlike the organization of the various army branches, did not follow the Prussian concept. The infantry, for example, early in the 1870s adopted (apart from its rejection of the German induction term of three years) the Prussian military structure. In early 1873 the *Conseil Supérieur de la Guerre* became deadlocked on the technical question of whether to have twelve army corps of three divisions each or, as in Germany, eighteen corps of two divisions each. President Thiers favored the former proposal and Marshal MacMahon the latter. One of the most immediate and far-reaching effects of Thiers's dismissal from the presidency in May 1873 was that MacMahon, as his successor, could urge the German arrangement.³⁵ On the organiza-

³² "Rapport sur la cavalerie par le capitaine Koszutski," October 1879, AG Vincennes, 7 N 657.

³³ "Note relative à l'exportation des chevaux" (countersigned by Charles de Freycinet as minister of war), May 18, 1888, AG Vincennes, 7 N 2. Fragmentary statistics can be found in cartons 7 N 664 and 7 N 116.

³⁴ General Grandin to Minister of War Ferron, August 21, 1887, AG Vincennes, 7 N 45.

³⁵ Quite mistaken is the assertion that the passage of the military reform bill of 1873 was "an example of the close cooperation among government, parliament, and military circles"; La Gorce, *La République et son armée* (Paris, 1963), 17. See Allan Mitchell, "Thiers, MacMahon, and the *Conseil Supérieur de la Guerre*," *French Historical Studies*, 6 (1969): 232–52.

tional chart, the rest followed: corps, divisions, regiments, battalions, companies. The retrospective comment of one historian that French military planners "looked to see what the Germans did and thus adopted the four-company battalion" may put the matter somewhat too simplistically.³⁶ Still, the reorganization of the French infantry after 1870 replicated very closely the German system, evidently on the assumption that no other distribution of manpower would yield a comparable strength and consistency.

Yet, in establishing their system of high command, the French rejected the Prussian general staff as an appropriate model for the French État-Major. One obvious explanation for this divergence is that France simply had no Helmut von Moltke, the man who conceived, implemented, and personified the German staff system. But the reasons for not emulating his example were, of course, various and complex.

During the initial decade after the war, planners were preoccupied by the question of whether to retain a "closed" corps of staff officers or to adopt an "open" corps in the Prussian style. Specifically, the issue concerned the possible recruitment of staff officers from other branches of the service, as in Germany, or the continued cultivation of a separate, elite État-Major that was exclusively trained for staff work, as in the past. Critics of the existing system emphasized the tendency of French staff officers to become bureaucrats—sedentary, without practical experience among the troops, and consequently ill prepared to make a rapid transition in wartime from desk job to field command. The undeniable merit of such objections gradually eroded support for a "closed" corps, and the French adopted the principle of the German system, although not yet the practice, in 1880.³⁷

This reform coincided with formal creation of a new École Supérieure de Guerre in Paris, openly patterned after the famous Prussian Kriegsakademie in Berlin. The first commandant of the new academy, General Lewal, ordinarily equated military science with German procedures. Of course, "it is not just a matter of foreign imitation," he assured everyone, but "a return to sound French doctrines." Yet he went on to assert that Germany had been demonstrably more successful in applying those doctrines than France had—the essential lesson of 1870. Therefore, humbling as it might be for the French to admit their inferiority, they should abandon their "radically false" conceptions of military schooling and staff work. "The Germans have entered upon this path," Lewal concluded, "the only one that can lead to assured results."³⁸ This was the view that prevailed in 1880 when the superannuated École d'État-Major was closed, the new academy was opened, and the shift to an "open" staff system began.³⁹

³⁶ Ralston, *The Army of the Republic*, 59–60.

³⁷ *Réorganisation du corps d'état-major: Projet présenté par la minorité de la commission de réorganisation* (Paris, 1872); and General Fave, "La Stratégie et le corps d'état-major," *Journal des sciences militaires*, 8th ser., 20 (May 1878): 135–40. For background, see Pierre Chalmrin, *L'Officier français de 1815 à 1870* (Paris, 1957); François Kuntz, *L'Officier français dans la nation* (Paris, 1960); and William Serman, *Les Origines des officiers français, 1848–1870* (Paris, 1979).

³⁸ Lewal, *La Réforme de l'armée* (Paris, 1871), *Études de guerre* (Paris, 1873), and "Tactiques de combat," *Journal des sciences militaires*, 8th ser., 7 (March 1874): 321–57.

³⁹ See Robert Laulan, *L'École militaire et l'école supérieure de guerre* (Paris, 1937).

Nevertheless, the French high command did not resemble the German in one important and familiar respect. In Germany the political and military regimes were organically united in the person of the Kaiser. Despite his sometime disputes with Bismarck, Moltke enjoyed a direct and usually harmonious relationship to his monarch, who reciprocated by granting considerable autonomy to the general staff in matters of military organization and planning. For these cozy arrangements there was no counterpart in republican France. The French president possessed neither William's clear constitutional authority nor his autocratic prerogatives. Political logic therefore dictated that the French *État-Major* be subject to direct civilian control exercised through the cabinet system. When compared with the German example, however, this had at least three important disadvantages: the *État-Major* was adversely affected by constant political instability; it remained suspended in ambiguity between its peacetime and wartime assignments; and it was left to function primarily as a bureaucratic adjunct of the Ministry of War, rather than as an independent agency whose task it was—as in imperial Germany—to prepare the nation for the eventuality of war.⁴⁰

The dangers of the German system, so amply revealed in 1914, require no elaboration here. Yet, largely through inertia, the French perpetuated many of their own worst faults. After the war of 1870, the French general staff was hardly a dynamic force for military regeneration. No doubt the French army possessed an abundance of intelligent and capable officers, but they were locked into a system that continued to encourage routine and was slow to learn from defeat. In the *État-Major*, imagination did not come to power.

THE TOTAL RECORD OF REFORM in the French army between 1870 and 1890 shows that the French effort to compete with the Germans by imitating them was hesitant, incomplete, and only sporadically successful. The heretofore classified documents housed in Vincennes support that observation far more than does the patriotic rhetoric of the public record upon which historical analyses have usually been based. Charles de Freycinet's confidential remark to General de Miribel that the escalation of German military prowess had placed France in "a situation of inferiority" was not exceptional within the inner sanctum of the French army. Actually, it expressed a view widely held among responsible military experts. In order to grasp the mood and the *mentalité* of the French at the onset of the Dreyfus Affair in the early 1890s, it is important to comprehend this prevailing malaise. The dominant emotion among most army officers was not revenge but a sense of failure, a feeling not of apathy but of fear. In the ineluctable competition with Germany, they had by their own admission fallen far short. After two decades, parity scarcely seemed closer, and they had reason to dread the outcome of an eventual military conflict.⁴¹

⁴⁰ The literature is too vast to cite fully here. Especially see Gerhard Ritter, *Staatskunst und Kriegshandwerk: Das Problem des "Militarismus" in Deutschland*, 4 vols. (Munich, 1959–68), 2: 11–43; and, for reviews of recent German military literature, Manfred Messerschmidt, *Militär und Politik in der Bismarckzeit und im wilhelminischen Deutschland* (Darmstadt, 1975); and Ralston, *The Army of the Republic*, 138–200.

⁴¹ See Allan Mitchell, "The Xenophobic Style: French Counterespionage and the Emergence of the Dreyfus Affair," *Journal of Modern History*, 52 (1980): 414–25.

Such a conclusion makes more understandable the urgency with which the French high command regarded an entente with Russia. Other explanations have been advanced for French motives in seeking an alliance with the tsarist regime. According to the traditional canons of diplomatic history, leaders of the Third Republic were eager to create a two-front dilemma for Germany; they thus deliberately risked a solidification of European alignments in order to tip the balance of power in their geopolitical favor and, perhaps, to hasten the day when the restitution of Alsace and Lorraine might become a realistic possibility.⁴² Another argument holds that French politicians acted primarily for domestic rather than foreign policy reasons: they sought to divert attention from accumulating internal conflicts and scandals or to protect substantial public investments in Russian bonds.⁴³ In fact, these contentions are neither implausible nor incompatible. But their respective advocates tend to strain unduly to make them exclusive. Specifically, they understate the most simple and compelling of explanations—that is, that the underlying French motive for a military agreement with Russia was military. The alliance, in effect, brought to the French the one thing they most conspicuously lacked: a large mass of armed reserves. Statistically, France thereby obtained at last a real, not a fictional, advantage over Germany in manpower and armament.

Yet, the Franco-Russian entente could not spare France from an initial German onslaught in 1914. A statistical edge meant little if troops and equipment could not be rapidly and effectively mobilized; the preponderance gained by the entente powers only held for a more protracted conflict. In view of Russian inefficiency, then, the French were constrained to hold their defensive posture and await a German invasion. Lest that seem self-evident now, it was not then. Professors of military science at the École Supérieure de Guerre tirelessly propounded the virtues of offensive warfare.⁴⁴ Even if inspired by these views while at the academy, however, after their graduation French cadets faced the less thrilling realities of geography, finance, inertia, and immediate German superiority. Hence, young officers soon found themselves attempting to plug a leaking dike. Improvements in transportation and technology dictated that fortresses be extensive, massive, and self-contained. But constructing enough bastions to form an effective barrier along two fronts was well beyond the means of a young republic struggling through the effects of an economic depression.⁴⁵ Moreover, the

⁴² See William L. Langer, *European Alliances and Alignments, 1871–1890* (2d ed., New York, 1964), 459–506, and *The Franco-Russian Alliance, 1890–1894* (2d ed., New York, 1967). For a recent commentary on the work of Langer, Kennan, and others, see James Joll, “The Old Diplomacy,” *New York Review of Books*, January 24, 1980, pp. 33–36.

⁴³ Joll, “The Old Diplomacy.” French financial speculations in Russia have a long history and a rich literature; especially see Rondo E. Cameron, *France and the Economic Development of Europe, 1800–1914* (Princeton, 1961), 421–24; and René Girault, *Emprunts russes et investissements français en Russie* (Paris, 1973). For the German counterpart to this argument, see Hans-Ulrich Wehler, “Bismarcks Imperialismus und späte Russlandpolitik unter dem Primat der Innenpolitik,” in Michael Stürmer, ed., *Das kaiserliche Deutschland: Politik und Gesellschaft, 1870–1918* (Düsseldorf, 1970), 235–64.

⁴⁴ Copies of lectures at the military academy may be found in the library there. For a critique of them, see Eugène Carras, *La Pensée militaire française* (Paris, 1960), 278–84. The contingency of a counteroffensive thrust by the French into the vicinity of Metz and Strasbourg was incorporated by military planners in the late 1880’s but was not emphasized until after 1910.

⁴⁵ Charles de Gaulle greatly exaggerated the successful completion of France’s “grandiose” defensive system; see his *La France et son armée* (Beyrouth, 1943), chap. 6: “Vers la revanche,” esp. 223.

disadvantages of tying down large garrisons in static defensive positions were increasingly apparent to French planners, and the adoption of a more mobile defensive strategy therefore became imperative. Still, to pass from that notion to a vision of offensive success, as did General Joffre on the eve of the First World War, was a clear case of misguided enthusiasm. France's best—perhaps only—chance to halt a swift German invasion was to rely on a combination of sheer luck and extemporaneous tactics. For that, the frantic chase of Parisian taxis to the Marne in 1914 remains a fitting, if mythic, symbol.⁴⁶

In the Great War, after all, France was saved by improvisation more than by planning, by dirt trenches more than by concrete fortifications, by German faults more than by French virtues, and by the heroism of soldiers more than by the foresight of generals.

⁴⁶ For a telling critique of Joffre's belated strategic innovations, see Samuel R. Williamson, Jr., *The Politics of Grand Strategy: Britain and France Prepare for War, 1904-1914* (Cambridge, Mass., 1969), 205-06. For an emphasis on Joffre's tactical skills as well as on the symbolic importance of the Parisian taxis, see Henry Contamine, *La Victoire de la Marne* (Paris, 1970), 82-102, *passim*.

Annaliste Paradigm? The Geohistorical Structuralism of Fernand Braudel

SAMUEL KINSER

IF THE NOBEL PRIZE WERE GIVEN TO HISTORIANS, it would almost certainly have been awarded to Fernand Braudel. International recognition of this French historian has increased enormously since the publication in English translation of his chief work, *The Mediterranean and the Mediterranean World in the Age of Philip II* (1972–73),¹ and he is no less renowned because of his leadership of the so-called *Annales* school.² The *Annales* group is “the most productive and lively school of historians practicing their art today,” concluded J. H. Hexter in 1972, and *La Méditerranée et le monde méditerranéen à l’époque de Philippe II* has been the “seminal work” for that school over “the past two decades.”³

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¹ Both editions of Braudel’s *La Méditerranée et le monde méditerranéen à l’époque de Philippe II* (Paris, 1949; 2d ed. in 2 vols., Paris, 1966) will be cited in the French editions throughout; I have made my own translations from the French for exactitude. (For an English translation of the second edition, see Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II*, trans. Siân Reynolds, 2 vols. [New York, 1972–73].) Unless otherwise noted, all italic material indicates Braudel’s emphasis, not mine. All citations to *La Méditerranée* include within parentheses the number of the edition. The original title of the journal *Annales: Économies, sociétés, civilisations* was *Annales d’histoire économique et sociale* (1929–39); during World War II it was published as *Annales d’histoire sociale* and *Mélanges d’histoire sociale* (1940–45). Unless otherwise specified by the use of a term like *Annales* paradigm or *Annales* school, the use of *Annales* refers to this series of publications from 1929 to the present.

² Lucien Febvre’s energy, prestige, and ministerial connections were largely responsible for obtaining the foundation in 1947 of the Sixth Section of the École Pratique des Hautes Études, devoted to economic and social science. Under his presidency and that of Braudel, who succeeded Febvre in 1956, the Sixth Section grew rapidly and in ways highly favorable to research of the *Annaliste* kind. Braudel cited the figure “eight hundred to a thousand” volumes of history published by writers of *Annaliste* bent since 1951; Braudel, Speech at the Inaugural Conference of the Fernand Braudel Center for the Study of Economies and World Systems, Binghamton, New York, May 1977. A figure of “five hundred to six hundred” teachers and scholars attached to various institutions of research in Paris, selected and funded because of *Annaliste* support, was given to me by several members of the current editorial board of *Annales* who were present at this conference. In 1972–73, the former Sixth Section, now the École des Hautes Études en Sciences Sociales, offered “forty-nine research seminars in History, all inclined in the *Annaliste* direction”; J. H. Hexter, “Fernand Braudel and the *Monde Braudellien* . . .,” *Journal of Modern History* [hereafter, *JMH*], 44 (1972): 497. But, of course, the mainstay of this powerhouse of historical scholarship is *Annales* itself, an international journal that since the mid-1960s has published two hundred pages of articles, research notes, and reviews every other month.

³ Hexter, “Fernand Braudel and the *Monde Braudellien*,” 529. For Troian Stoianovich, Braudel’s involvement with the “*Annales* movement” makes him not only its most knowledgeable spokesman but even the “personification” of the school; Stoianovich, *French Historical Method: The Annales Paradigm* (Ithaca, N.Y., 1972), 22.

In the eyes of its author, Braudel's history is, above all, structural history.⁴ The *Annales* "paradigm,"⁵ at least during the years of Braudel's leadership (ca. 1950–70), thus seems to be related to the structuralist models that have captivated French social science. But this interpretation is misleading. There is little connection between the "structuralism" to which Braudel admiringly referred when he issued a second edition of *La Méditerranée* in 1966 and his concept of structure when he first wrote *La Méditerranée* two decades earlier. Braudel's fellow historian Ernest Labrousse has emphasized the relation of Braudel's work to the old historiographical tradition of geohistory rather than to the new vogue of structuralism.⁶ Indeed, in the first edition of *La Méditerranée*, Braudel's "structural history" and geohistory have common ideological roots. But in the second edition, partly in response to the new vogue of structuralism, his idea of structure shifted in order to accommodate what he called the economists' "true distinction" between "structure" and "conjuncture."⁷ This accommodation was, however, limited, and it raised as many problems as it solved. In what sense, then, may *La Méditerranée* be called paradigmatic for those who have pursued social scientific history in the mode of the *Annales*?

The analytic form or "methodology" of any science may be said to have three aspects: ideological, heuristic, and experimental. In probing the question of the paradigmatic quality of *La Méditerranée*, I will concentrate on heuristic and ideological questions of methodology. The *ideological* aspect concerns the conceptualization of the object of study. In the historian's case, this object is at once concrete and abstract; it involves not just specifying particular human actions, collective or individual, but also ascertaining the continuity or discontinuity among them. In fact, the way in which time "passes" smoothly or roughly, in discontinuous jumps or in continuous waves, across or through or perhaps by means of first one type of human activity (economic, cultural, and so on) and then another, is always implied, latently or explicitly, in the historian's descrip-

⁴ In 1966, Braudel summed up his method, "I am a 'structuralist' by temperament, little intrigued by the event and only half-way engaged by the conjuncture, that grouping of events of the same kind"; *La Méditerranée* (2d ed.), 2: 520.

⁵ The object of Hexter's and Stoianovich's works is to isolate, in Hexter's words, the "paradigms . . . that give French historical scholarship its particular posture and quality"; "Fernand Braudel and the *Monde Braudellien*," 481. For Stoianovich's estimates of *La Méditerranée*, see his *French Historical Method: The Annales Paradigm*, 66–70, 114–16, 167–69. Also note Lucien Febvre's comment in 1950 that Braudel's work is the "embodiment" of the *Annaliste* "image of history"; Febvre, "Un Livre qui grandit," *Revue historique*, 203 (1950): 224. Ernest Labrousse reported that he, as the person presenting Braudel's thesis to the Sorbonne jury in 1947, wrote that Braudel's work would "mark an epoch in world historiography," and that the jury in its report on the thesis repeated "almost literally" these words; Labrousse, "Aux vingt-cinq ans de *La Méditerranée*," in *Histoire économique du monde méditerranéen, 1450–1650*, volume 1 of *Mélanges en l'honneur de Fernand Braudel* (Toulouse, 1973), 11. The professional response to *Annaliste* historiography by historians in England and America has not always been couched in this encomiastic vein. For a review of many of these negative responses that have come particularly from economic and social historians (Richard Cobb, Richard Herr, David Pinkney, J. H. Plumb, and Isser Woloch, among others), see Stoianovich, *French Historical Method: The Annales Paradigm*, 232–35. Also see the recent criticisms of Bernard Bailyn and Robert Forster; Bailyn, Review Article of Stoianovich, *French Historical Method: The Annales Paradigm*, in the *Journal of Economic History*, 37 (1977): 1028–34; and Forster, "The Achievements of the *Annales* School," *ibid.*, 38 (1978): 58–75.

⁶ Until his retirement in 1966, Labrousse held the chair of economic history at the Sorbonne (the only such chair in France), which Marc Bloch had earlier occupied. To Labrousse, Braudel's geohistorical mode of combining "intersecular," "international," and "interdisciplinary" approaches to human activity "symbolized with unequaled brilliance" the novelty of the history promoted by the *Annales*; "Aux vingt-cinq ans de *La Méditerranée*," 12, 17.

⁷ Braudel, *La Méditerranée* (2d ed.), 1: 325.

tions of places and persons. The *heuristic* aspect of analytic form concerns the modes of locating and delimiting the object of study, such that others can replicate the historian's process of finding and researching relevant materials.

The *experimental* aspect concerns the applicability of the historian's conceptualization. More or less explicitly, as is the case with the other two aspects, the scope of the actual inquiry of the historian, the "experiments" performed on a variety of sources, is related to the scope of all possible inquiries of this ideologically and heuristically defined type. Except insofar as it affects the latter, I will not discuss the book's experimental value for particular branches of study, like the urban history of Italy or the military policies of Philip II. In the course of my discussion, I will pay some attention not only to research strategies but also to modes of presenting the research, for the two are intertwined: the verbal formulas used to describe an object also create it, even help discover it, long before the formulas are put together in narrative, historiographical form.

In essays that Braudel published between 1950 and 1960, he frequently discussed ideas of structure in terms of their implications for historical method from heuristic and ideological points of view. But Braudel did not relate these discussions to experimental practice except insofar as he referred for illustrative purposes to work published elsewhere. Thus, his remarks cannot be taken as blueprints for the historical method practiced in either the first or the second edition of *La Méditerranée*, although these essays do help in understanding what Braudel set out to do in revising that work along the lines of economic structuralism.⁸

In giving my essay a roughly chronological form moving from examination of the sources of Braudel's Mediterranean project in the 1920s to the changes in the book's conception in the 1960s, I am less interested in establishing the author's own originality or his indebtedness to others than in discerning how *La Méditerranée* is related to the *Annales* movement. Indeed, the question of Braudel's structuralism can best be pursued in relation to the more general possibility of developing structural methods in history. This doubly enlarged focus has a mutually clarifying result: the meaning of Braudel's central methodological term, "structural history," becomes clear when it is seen not simply in relation to the *Annales* school or to the contemporary structuralist movement but also in relation to the general movement of French historical and social-scientific thought in the nineteenth and twentieth centuries.

La Méditerranée IS DIVIDED INTO THREE PARTS, which Braudel described in the first edition as dealing with three kinds of time: geographical, social, and individual.

⁸ For the most significant of these essays, see Fernand Braudel, "Pour une économie historique," *Revue économique*, 1 (1950): 37-44, "Les Responsabilités de l'histoire," *Cahiers internationaux de sociologie*, 10 (1951): 3-18, "Georges Gurvitch ou la discontinuité du social," *Annales*, 8 (1953): 347-61, "Histoire et sciences sociales: La Longue durée," *ibid.*, 13 (1958): 725-53, "Histoire et sociologie," in Georges Gurvitch, ed., *Traité de sociologie*, 1 (Paris, 1958): 33-98, "L'Apport de l'histoire des civilisations," *Encyclopédie française*, 20 (Paris, 1959): chap. 5, and "La Démographie et les dimensions des sciences de l'homme," *Annales*, 15 (1960): 493-523. Many of these essays have been reprinted in a collection of Braudel's articles, *Écrits sur l'histoire* (Paris, 1969); all succeeding references to these essays use the page references in this volume.

Three types of historiography correspond to the study of these three kinds of time. The first type, forming the "geohistory" of Part 1, seeks to grasp an "almost immobile history of man's relations with the milieu surrounding him; the second, to represent "a slowly rhythmic history . . . of groups and groupings" of people; and the third, to portray a "history of short, rapid, nervous oscillations" of "traditional," "eventful history," which in Braudel's Part 3 consists of the twists and turns of politico-military history. The order of the three histories, ranged in diminishing importance, emphasizes Braudel's lack of interest in the picayune history of diplomacy, "shuttered up in its chosen area" and refusing to look "beyond the diplomatic files to real life, fertile and promising." Echoing the polemics of Lucien Febvre and Marc Bloch, Braudel described how, as he worked on the politically oriented doctoral thesis that later became *La Méditerranée*, he found himself irresistibly drawn to the study of a "new kind of history," *Annaliste* history, "economic and social history of a revolutionary kind."⁹

The economic and social history presented in the first edition of *La Méditerranée* is not, however, particularly "revolutionary." The "new" history to which Braudel lent his pen was in fact old by the time *Annales* was founded in 1929. With respect to "geographical time," Braudel's conceptual guide was the work of Paul Vidal de la Blache (1845–1918), the master of French "human geography" and an important mentor of Braudel's patron, Lucien Febvre. With respect to "social time," Braudel's model was the work of Henri Pirenne (1862–1935). Braudel did not, in *La Méditerranée*, follow the newer approaches to economic and social history embodied in Bloch's and Febvre's work. He chose not to refer to Bloch's innovative analyses of the agricultural patterns of production prevailing in Europe during medieval and early modern times. Nor did he make use of Febvre's idea that religious, literary, and cultural activity is coerced and guided by "mental tools" characteristic of a given time-space.

Braudel thus ignored what seemed to many historians in the 1940s to be the most significant historiographical achievements of the first two editors of *Annales*. But he echoed and extended their most signal ideological directive, the synthesizing of history with other social sciences. With fewer doubts and questions than Bloch and Febvre had, Braudel affirmed that history is a scientific enterprise, sharing with other social sciences a single, identifiable goal that is progressively achievable:

Instead of traditional historical geography . . . devoted almost solely to the study of state frontiers and administrative boundaries . . . , to create a truly retrospective human geography . . . : such would be the ambition of this *geohistory* . . . ; such is . . . its true *raison d'être*, the justification of its action in favor of the convergence of two social sciences, history and geography, which find no advantage in being separated from each other. To the contrary, the purpose of all the social sciences is the same, over and above our tools and our different workplaces. It is man, and always man, which one must apprehend—man, which is to say society, states. One may use time or again space in a more special way . . . [but] Time and Space are only means.¹⁰

⁹ Braudel, *La Méditerranée* (2d ed.), 1: 15–17.

¹⁰ *Ibid.* (1st ed.), 296; the ellipses in the last line, after "special way," are Braudel's.

A social science, as opposed to the outworn history of diplomatic events and political leaders, collectivizes its object. Collective man is, for Braudel, anonymous man. The states and societies that are "man" are themselves collectivized in Braudel's writings so as to give them a single, general form. Braudel sought a socially embedded but naturalistically generalized man, and the Mediterranean is a privileged area in which to pursue the search because behavior there is exemplary for humanity generally. "The Mediterranean . . . is a collection of museums of Man. . . . One finds there a human milieu, an accumulation of men which the noisiest, most spectacular invasions have shown themselves incapable of affecting deeply." The Mediterranean area thus became for Braudel a splendid showcase of what is humanly possible. "The Mediterranean . . . is a meeting-place, an amalgam, a *human unity*."¹¹

This humanistic-naturalistic idea of man¹² treats society not as a diversity of groups but as a single whole, everywhere the same although exemplified with greater or lesser clarity in different locales. Braudel affirmed that individual actions were both infinitely varied and yet all similar in basic ways. What are more difficult to conceptualize in terms of this humanism are historically efficacious differences at the social level, the clashing or cooperation of social groups. Three grand abstractions are granted equal status in Braudel's historiography: Space, Time, and Man. Space is usually analyzed as a series of socially influenced ecological systems, and Time is a multiplicity of "levels" with different "rhythms." Man appears as the intersection of this Time and this Space, since they work upon human endeavor to average out individual cases. The longer the span of time and the larger the space, the more the collectively differentiated efforts of individuals, articulated in different social groups, seem to lose their specificity as well; what one class or one nation accomplishes is finally subject to the iron rule of environment and to the habits, infinitely repeated, that are engendered by this environment. Thus, Man's "unity" implies social and individual anonymity: "I am tempted, confronted by man, always to see him enclosed in a destiny that he scarcely helps fabricate, in a landscape that stretches before and after him in the infinite perspectives of 'long duration.'"¹³

No longer self-determining or even collectively determining, the human individual shrinks and fades away in Braudel's pages before the grandeur of the environment. But the vibrant force of individuality somewhat paradoxically reappears upon the stage of history masked as this same more-than-human power. The chapters of *La Méditerranée* devoted to geohistory, Braudel stated in the first edition, "are intended to remind the reader that . . . behind all of human history there is this actor—an actor who promptly transforms himself, who is always

¹¹ *Ibid.*, 298, 195. These sentences were deleted in the second edition.

¹² The term "humanistic-naturalistic" emphasizes that Braudel's idea of man derives from Enlightenment naturalism and not from the supernaturally oriented humanism of Renaissance tradition.

¹³ Braudel, *La Méditerranée* (2d ed.), 2: 520. These assumptions concerning the unity of the historical object reinforce the *Annaliste* doctrine that all social sciences can and should converge in method and purpose. As Febvre put it, in reviewing the contents of a book celebrating the foundation of the geographical laboratory at Rennes, "[Ce sont toujours des] questions de méthodes et de liaison . . . celles de la géographie avec l'histoire, la sociologie, l'ethnographie, la linguistique, la psychologie—d'un mot avec l'Homme, singulier collectif pour dire 'les hommes'"; Lucien Febvre, "La Géographie: Réflexions sur un cinquantenaire," *Annales*, 8 (1953): 372.

adroit, who always presses himself forward, and who is often decisive in his intervention. What shall we call him? Space? The word says too little. The earth? An equivocal name. Let us say the geographical milieu."¹⁴ This double switch among two of Braudel's most general categories—man as a mere locus of determination, an intersection of time-space, and space as a human actor energetic and prompt to change costume—is a radicalization of the habitually metaphorical treatment of abstract nouns and collective names by Braudel's geohistorical mentor, Paul Vidal de la Blache.

Long before Braudel, Vidal had treated geographical spaces as living beings. "We willingly repeat the words of Michelet: 'France is a person.'" He continued, "This word, personality, belongs to the domain and vocabulary of human geography. It corresponds to an advanced degree of development. . . . Our country emerged earlier than others from that vague rudimentary state . . . where nothing yet . . . suggests a living personality."¹⁵ These words from the man who in Braudel's opinion wrote "the densest text that has ever been written on the being and destiny of the Interior Sea" go far to explain Braudel's habit of treating not merely the sea but also the temporal rhythms manifested in events as if they were living beings, organisms, "personages" of varying power and influence.

For Vidal, French soil was, like the nation generally, a "historical personage." The soil "acts by means of the pressure that it exerts on habits and by means of the resources that it supplies in times of misery; it regulates the oscillations of our history."¹⁶ Vidal developed a watery metaphor to emphasize "what is fixed and permanent" about the historical geography of France: "When a gust of wind agitates with violence the surface of a clear body of water, everything is shaken and intermingles; but after a moment the basic image outlines itself once

¹⁴ Braudel, *La Méditerranée* (1st ed.), 3. Febvre, who introduced Braudel to the editorial staff of *Annales* and guided his career in so many other ways, also treated abstract nouns in ways similar to those in this passage. He introduced his own doctoral thesis, "Nous nous sommes proposé, dans le travail qui va suivre, d'étudier la vie intérieure d'une individualité politique: la Franche-Comté, pendant une des périodes les plus vivantes de son histoire, la seconde moitié du XVI^e siècle. . . . Ce qui nous intéresse, c'est moins une certaine région à une certaine date qu'à un moment déterminé de son évolution une *personne* historique collective, trouvant dans un État son *expression* politique." Lucien Febvre, *Philippe II et la Franche-Comté* (1912; 2d ed., Paris, 1970), 7 (italics added). On Febvre's organic-humanistic rhetoric, see Hans-Dieter Mann, *Lucien Febvre, La Pensée vivante d'un historien* (Paris, 1971); and R. Chartier and J. Revel, "Lucien Febvre et les sciences sociales," *Historiens et géographes: Revue de l'association des professeurs d'histoire et de géographie de l'enseignement publique*, no. 272 (1979): 427–42.

¹⁵ Vidal de la Blache, *Tableau de la géographie de la France*, volume 1 of Ernest Lavisse, ed., *Histoire de France* (Paris, 1911), 7–8. Braudel cited Vidal's work in general as "one of the most fruitful . . . , perhaps even the most fruitful of any," for the discipline of history, and he singled out this background volume of the famed *Histoire de France* among Vidal's works in his inaugural lecture at the Collège de France in 1950 as "one of the major works not only in the geographical tradition [*l'école géographique*] but also in the French historiographical tradition [*l'école historique française*];" Braudel, "Positions de l'Histoire en 1950" (originally published as "Les Responsabilités de l'histoire"), in his *Écrits sur l'histoire*, 31. But Vidal wrote a short study of the Mediterranean region as well, about which Braudel commented with regard to his own book, "The pages over which I have lingered most, perhaps, are those that Vidal de la Blache has consecrated to the Mediterranean. . . . They have such a richness, exactitude, and force of thought that reading and rereading . . . reveal more and more"; *La Méditerranée* (1st ed.), 1125. Braudel deleted the second sentence, however, from the second edition; *ibid.* (2d ed.), 2: 543.

¹⁶ "The earth remains in France the nurse of her children," wrote Vidal, and new currents of economic change should not disguise such "fundamental facts" (*le fond des choses*); *Tableau de la géographie*, 384. In like manner, Braudel defined the geographical part of *La Méditerranée* as "history tied to the soil"; *La Méditerranée* (1st ed.), xiii.

more." Using similar metaphors, Braudel explained how the "almost immobile" geohistory of Part 1 lies beneath a "slowly rhythmic" social history, whose "deep currents [*vagues de fond*] raise the totality of Mediterranean life." Extending the image to a three-part differentiation, he described the "eventful history" of Part 3 as a "surface agitation, waves that the tides raise with their powerful movement." The three histories of which *La Méditerranée* is composed are related to each other like depths and heights of moving, fluid substance.¹⁷

Braudelian geohistory is based on three assumptions, all of which he shared with Vidal: (1) geohistory has a specific, concrete object that is "tied to the soil," to down-to-earth, elemental, ecological conditions; (2) geohistorical process, because it develops slowly, represents a relatively "immobile" history, whose characteristic patterns last for long periods; and (3) geohistory is fundamental to other kinds of historical processes and underlies other forms of historicity. The tendency toward environmental determinism, recurrent in geohistorical writers from Polybius to Henry Thomas Buckle and Vidal, is involved in the last assumption, and Braudel discussed that tendency in the concluding section of Part 1, entitled "Geohistory and Determinism."

In this section, Braudel deftly affirmed and yet simultaneously denied determinism. Within the bounds of technological capacity, he wrote, "man" is free to do what he will with the landscapes in which he dwells. But the very formative capacity of human endeavor, the ability to bend natural forces in a serviceable direction and to shape the environment in a certain way, creates constraints that become in turn determinants of human will because they are relatively more "fixed" and "permanent."¹⁸ The "great man of action," Braudel concluded in the second edition, "is he who weighs the narrowness of his possibilities with exactitude, choosing to stay within them and even to take advantage of the weight of the inevitable so as to add it to his own efforts." Unlike Machiavelli's doctrine of greatness, which similarly acknowledges the prime importance of judiciousness about the general run of affairs while it also urges that a prominent politician must, in order to exercise this very judiciousness, know how to turn particular contingencies to his profit and thus "overcome Fortune," Braudel's conclusions seem to deny to short-term adroitness any means of pulling awry the proper course of long-term change. "All effort against the prevailing direction of the deep meaning of history [*à contre-courant du sens profond de l'histoire*] . . . is condemned in advance." Thus, what is "richest in humanity"—the story of "individuals" with its quick rhythms—is an entirely ephemeral and "short-sighted"

¹⁷ Vidal, *Tableau de la géographie*, 386; and Braudel, *La Méditerranée* (1st ed.), xiii, and *ibid.* (2d ed.), 2: 543.

¹⁸ Braudel, *La Méditerranée* (1st ed.), 295–304. To understand Braudel's interest in the problem of geohistorical determinism and the sophistication with which he deals with the problem of individual and short-term freedom versus collective and long-term determination, some knowledge of Febvre's geohistorical thought is necessary. Febvre's position has been called a "geographical possibilism." More sanguine than Braudel about the capacity of men to change their environment, Febvre was also more insistent than Vidal on the progressive historical steps by which a landscape is adapted to human needs. Febvre's most important work on this subject is *La Terre et l'évolution humaine: Introduction géographique à l'histoire* (Paris, 1922), in which he attacked the geographical determinism of the German school led by Friedrich Ratzel. Also involved in the problem of human and environmental interaction as it presented itself to Braudel is the set of organic presuppositions—presuppositions particularly fashionable in French social science at the time Vidal wrote—that are analyzed later in this essay; see pages 72–73, below.

affair.¹⁹ The apparent free will of this puny actor, man, only serves to augment the power of his giant adversaries upon the stage of history, for what is formed by human action becomes fixed in its rhythms, turns on its maker, and subordinates man to its grand, underlying currents.²⁰

The ebullience of Braudel's rhetoric gives sweep and grandeur to a historiographical schema that is simply, reiteratively designed. Thus, although his "means of exposition," as he called them in the preface to his first edition, "switch . . . from one to another," they consistently personify, they regularly set individual initiatives in frameworks that render the effects of action anonymous, and they always seek to portray abstract, general forces in concrete, colorful terms. "We have arrived at a decomposition of history into planes of different degrees [*plans étagés*]" (presumably, the levels of the "almost immobile," of "deep currents," and of "waves"), at a decomposition of "historical time" into "geographical time, social time, and individual time," and at a "decomposition of man into a procession of personages [*cortège de personnages*]."²¹ Levels, times, personages; Space, Time, Man. Braudel's movement from one means of exposition to another does not alter the framework of his discourse but, rather, proceeds circularly among its metasigns.

PART 1, WITH ITS DEEP HISTORY OF MAN interacting with a personalized landscape, and Part 3, with its superficial history confined to individually guided short-term changes, posed few problems of conception for the author. Part 2 was another matter. Here Braudel proposed to deal with "Collective Destinies and General Movements" (*Destins collectifs et mouvements d'ensemble*). "At this level, all forms of life interact," wrote Braudel. "And the difficulty lies in giving each its due share, in knowing how they act and react on each other." Part 2 might have been called "a social history, that of groups and groupings" of men, Braudel concluded, had not the term "social history" been "diverted from its full meaning." As an alternative Braudel turned toward the temporally abstracting terms of a "slowly rhythmic history" referred to as "the structural history of Gaston Roupnel."²²

In the first edition of *La Méditerranée*, the geohistory in Part 1 was differentiated by Braudel from the history of structures in Part 2.²³ Nevertheless, the ideological assumptions that give substance to geohistory and "the structural history of Gaston Roupnel" are similar. Roupnel (1871–1946) outlined his idea of "structural history" in *History and Destiny*, which Braudel reviewed enthusiastically for the wartime *Annales*. Braudel noted that Roupnel had written about

¹⁹ In chapter 25 of *The Prince*, Machiavelli maintained that men, to be successful in their affairs, must, above all, "move in tune" with the times and yet they should also act impetuously, since "fortune is a woman" and can be overcome. For Braudel's remarks about the great man of action, see *La Méditerranée* (2d ed.), 2: 520.

²⁰ Braudel, *La Méditerranée* (2d ed.), 1: 16.

²¹ *Ibid.*, 17.

²² *Ibid.* (1st ed.), 307, xiii.

²³ Braudel repeated the distinction in a one-page introduction to Part 2: in the first part, narration is said to move from "things" to "man," and, in the second part, from "man" to things like the "history of groups, of structures"; *ibid.*, 307.

the role of “structural history” in the same terms as Vidal had about geography: “The history of a people,” wrote Roupnel, “is determined . . . at the level of the soil, in its down-to-earth life.”²⁴ It was, no doubt, Roupnel’s earthy, popular-national, and sensorily concrete orientation to historical inquiry that accounts for the appeal that Roupnel’s verbal formula, “structural history,” had for Braudel.²⁵ Braudel wrote his review—and, for that matter, most of *La Méditerranée*—while interned in a German prison camp, and the circumstances of the war undoubtedly made him more responsive to a work that associated “history” with “destiny” rather than with events or “occurrences.” As he commented some thirty years later, Braudel felt he “had to outdistance, reject, deny” the events that poured in from “the radio and the newspapers.” “Down with occurrences,” he wrote, “especially vexing ones! I had to believe that history, destiny, was written at a much more profound level. . . . So it was that I consciously set forth in search of a historical language . . . in order to present unchanging (or at least very slowly changing) conditions that stubbornly assert themselves over and over again.”²⁶ The emphasis of Part 2 on the determining effects of routes and the placement of cities, of the slowness of communication and transport, and of inflexible cultural frontiers expresses, among other things, nostalgia for a warmly remembered time, another kind of time—solid, enduring, and repetitive, immune from the degradations of World War II.

Braudel found in Roupnel’s work not only the values but also the “historical language” for which he was “consciously” searching. “If history were simply composed of events and sudden reversals,” Roupnel noted, “it would be nothing but a book of adventures.” But such “storylike history” (*histoire historisante*), “spaced out along time’s extensions,” develops “only along a horizontal plane.” Beneath “this superficial agitation” lies “the profound reality and muscular gestures of History.” Such a “reality” reveals itself—to those who can see with “clearsighted reflection”—“facts that unfurl themselves so slowly” that they seem to be actions of a “life” freed from the “circumstantial and individual.” These “facts,” which are “historical facts” in spite of seeming to be “without age

²⁴ Roupnel, *Histoire et destin* (Paris, 1943), 205, as quoted in Braudel’s review, “Faillite de l’histoire: Triomphe du destin?” *Mélanges d’histoire sociale: Annales d’histoire sociale*, 6 (1944): 72. I am not asserting that Braudel explicitly drew a parallel between Roupnel’s work and Vidal’s but simply that the terms that he used to describe Roupnel’s structural history are similar to terms employed by Vidal and himself. “The greatest satisfaction that this book brought me is those dense, intelligent pages that bespeak a history of depth and mass,” explained Braudel; Roupnel “entitles them rather nicely as ‘structural history’”; *ibid.* Braudel further quoted from Roupnel’s work a phrase that no doubt helped give him the idea of connecting “structural history” and “social history” in the first edition: “The public and social form of a country sketches its traits on the material image of fields and soil”; *ibid.*

²⁵ “Like Roupnel, the historian of the Burgundian countryside,” wrote Braudel, “I remain . . . a historian of peasant stock”; Braudel, “Personal Testimony,” *JMH*, 44 (1972): 448. Braudel referred to Roupnel as a historian of the Burgundian countryside because of Roupnel’s doctoral thesis, published as *La Ville et la Campagne au XVII^e siècle: Études sur les populations du pays dijonnais* (Paris, 1922). Pierre Goubert called this work “the first great book on rural history”; Goubert, *The Ancien Regime: French Society, 1600–1750*, trans. Steve Cox (New York, 1973), 15.

²⁶ Braudel’s reflections on his wartime writing are contained in an article written specifically for the issue of the *Journal of Modern History* honoring his work on the occasion of the appearance in English translation of the second edition of *La Méditerranée*. Despite the lack of books and research notes in the prison camp, Braudel wrote sections of his doctoral thesis, the first edition of that work, and sent the pages as he finished them to Febvre (1940–45). See Braudel, “Personal Testimony,” 453–54.

or history," are "structural facts" (*des faits de structure*). "They construct human society and maintain it in equilibrium and activity."²⁷

These concepts—Vidal's human geography, Roupnel's structural history, and Braudel's historical schema in the first edition of *La Méditerranée*—are expressed in rhetoric so similar as to make their formulations seem guided by a common semantic code. There is, for example, a shared contrast between depth and height: storylike history (Roupnel), the events of the current day (Vidal), and eventful history (Braudel) are superficial; real history lies deeper, at the level of the soil. Again, slow and fast tempos are used to oppose repetitious activities, those that have little temporally measurable change, and innovative activities, those that change rapidly: forces that remain (Roupnel), geography that regulates the oscillations of history (Vidal), and slowly rhythmic, deep currents (Braudel) are more important than "sensational events and dramatic reversals." Such contrasts are used to establish dependency, not polarity; the high rests upon the low, the fast is controlled by the slow: "structural facts" maintain society's equilibrium (Roupnel); "fundamental" factors, not the gusts of wind on the surface, determine affairs (Vidal); and geohistory, the slowest-moving history, is treated first in *La Méditerranée* because it is the base on which all else rises. Finally, all three authors used personification. The power of the structural level to control affairs parallels evocation of the nation as a "living personality" (Vidal); deep "History" constructs human society with its "muscular gestures" (Roupnel); and the Mediterranean Sea "dominates" historical action with its "grand presence" (Braudel).

Implicit in these formulations and, hence, controlling their expression is the logic of totality. The notion that in a given group of phenomena there is a whole that includes and orders all of its parts is essential to the idea of an organism. For that reason, with due regard to the tendency of all three authors to give a human form to the totalities in question, this shared rhetoric can be called organic-humanistic. An organism's parts act and react in accordance with the organization of the whole. By contrast, one part of a mechanism may be set in motion without involving other parts. An organism is said to possess autonomy because it is inwardly centered, operating outward from heart, brain, or soul. Mechanical entities have no center and hence no permanent "inside"; they possess only adjacent surfaces that interact. An organism's evolution is processive, connecting change in one part with change in all others; thus, an organism evolves toward greater complexity, greater inclusiveness, and ever grander totalization. Mechanical evolution is usually conceived as sequential rather than processive; what happens next to a mechanically articulated aggregate is conditioned by the sequence of what has happened before to the mechanical or organic entities with which it is in contact. But the order of this sequential conditioning is thought to be determined by adjacency rather than by inwardly centered organization.

The contrast between organic and mechanical behavior dates in the Western

²⁷ Roupnel, *Histoire et destin*, 174–75.

tradition from the fifth century B.C. at the latest. Given some methodological form by Democritus and Aristotle, that contrast has been basic to differentiating the “life” and “physical” sciences ever since. When Braudel chose organic metaphors to explain the larger features of historical change, he invoked a code with deep linguistic roots, the ramifications of which he could control only if he chose his words with care. Braudel, however, like his models Vidal and Roupnel and his mentors Febvre and Bloch, regularly used organic metaphors in a broad, unqualified way. He evidently found them so congenial that nuance or restriction in their use seemed to him unnecessary.²⁸

But, not surprisingly, Braudel did not feel as comfortable with the term “structure,” for twentieth-century structuralism has emphasized systematicity, mathematization, and logical implication and has tended to use materialistic-mechanistic analogies in developing these themes. The “structural history” that Braudel adopted from Roupnel values flexibility and descriptive concreteness above systematicity and abstract rigor, but these preferences cannot be clearly argued as long as the word’s double-edged possibilities are not explored and distinguished.²⁹ Braudel’s failure to undertake such an exploration led to elision of the difference between a pattern or systematized layout of empirically verifiable elements and the structure or systemic logic exhibited by such a pattern, an elision that became methodologically central to the “structuralism” of the second edition of *La Méditerranée*.

In the first edition, the ambivalences of structurality are explained as a heuristic rather than as an ideological difficulty. The depth of structural analysis made Braudel “uneasy.” Its distance from the perceptible surface of life, from concrete, living realities, made it suspect. “Structural history”—Braudel’s name

²⁸ In a lecture given in 1941, Lucien Febvre commented about the foundation of *Annales*, “Nous étions d’accord pour penser que, précisément, un mot aussi vague que ‘social’ semblait avoir été créé et mis au monde par un décret nominatif de la Providence historique, pour servir d’enseigne à une revue qui prétendait ne pas s’entourer de murailles. ... Il n’y a pas d’histoire économique et sociale. Il y a l’histoire tout court, dans son Unité.” Febvre, *Combats pour l’histoire* (Paris, 1953), 20. On Febvre’s humanistic-organic rhetoric, see note 14 page 68, above. Febvre in turn was strongly influenced by the humanism and organicism of Henri Berr on this point; see Martin Siegel, “Henri Berr’s *Revue de Synthèse Historique*,” *History and Theory*, 9 (1970): 322–34. Another important source for organic thinking in Braudel’s work and that of other *Annalistes* is Durkheimian sociology as modeled in the pages of *L’Année sociologique* (1900–), a periodical admired with some reservation by Bloch and Febvre. The outline of organic-mechanical differences offered here is, of course, incompletely sketched and, in particular, does not pretend to do justice to the many recombinations of the polarity that have occurred in Western thought. As early as Aristotle, for example, mechanism might refer not to the anti-organic extreme of materialistic-mechanistic assemblages (as in Democritan atomism) but to a “weakened” organism, such that a mechanism becomes a less complex form of reciprocally interrelated, hierarchically arranged, exactly interacting parts. This idea of the mechanical, fully developed in Descartes’s *Tractatus de Homine*, is thought out within the organic logic of completeness—that is, as Georges Canguilhem has emphasized, within terms of the ends, functions, and goal-oriented behavior for which mechanical assemblages serve as means. See Canguilhem, “Machine et organisme,” in his *La Connaissance de la vie* (Paris, 1975), 101–27.

²⁹ Derived from the Latin *struere* (“to build”), *structura* was used by Vitruvius and others to refer to the general form of walls or houses put together from “lifeless” material elements. But by the seventeenth century the word was applied to organic forms (the structure of a hand, of invertebrate animals, and the like). The semantic history of “structure” thus accords equally well with mechanical or organic interpretations of its meaning; as a result, there was ample linguistic space in which to conduct polemics against one-sided employment of the term by the structuralists in the “human sciences”; see pages 80–86, below. In fact, a detailed study of Braudel’s use of “structure,” not only in *La Méditerranée* but also in his essays on historical method, shows that he moved back and forth between mechanical and organic meanings of the term, for he sometimes supplemented his predominantly organic descriptions of structure as a “long-term reality” or “constraint” with references to it as a “mechanism.”

for “social history” in the first edition—“leads us over pathways that are too broad, beyond what we can perceive. One false step and a dizzying drop becomes possible.” Hence, Braudel continued, he has brought a “mass” of new documents to the analysis of what “lifted” Mediterranean space and set its social groupings in motion. “With reference to the deep and complex history of the sea, it was essential to bring forward new facts and, thanks to them . . . , dispel . . . the inexact explanations of so many historians.”³⁰

The penetrating power of a structural approach is dangerous to historians working in terms of paradigms of “living” totalities because its applicability and theoretical versatility threaten to dissolve all manageable limits for research. At the same time, the semantic code joining “structure” and concreteness, “dizzying” depth and surface detail, encourages the idea that, beyond the confusions displayed in documentary particulars, there must be a grander whole that reconciles all. The ultimate value of humanistic-organic coding is that it guarantees the unity of the historian’s object of study. This comforting guarantee offers the researcher a marvelous flexibility in the choice of heuristic procedures. Anything may be worth trying, because the results are certain to have some relation to Man. In a lecture delivered in 1950, one year after the first appearance of *La Méditerranée*, Braudel described in general terms the danger confronting the structural historian. Historians, he wrote, are involved in an “interminable” search for “new materials,” and the methods they apply to these materials are variable. “In ten or twenty years, our methods in economics and statistics will probably have lost some of their value, and . . . our conclusions will be contested and overturned. . . . [Thus] these materials and this information must be raised up and rethought on the human scale [*les soulever, les repenser à la mesure de l’homme*].” Indeed, “over and beyond their particularities it is a question, if possible, of finding *life* once more, of showing how these forces are tied up together . . . and how they also frequently mix together their turbulent waters.” It is, then, a question—and here Braudel brought together the ideological themes examined thus far with exemplary clarity—of “restoring *everything*, so as to resituate everything in the *general* framework of history, with the purpose of respecting—despite difficulties, antinomies, and basic contradictions—the *unity* of history that is the unity of *life*.”³¹

FOR BRAUDEL, THE “NEW FACTS” that rendered “structural history” somewhat less dangerous and more perceptible, concrete, and, hence, assimilable to an organic coding of general historical processes are, by and large, economic—facts about the amount, kinds, and geographical paths of trade in the Mediterranean area. The particular heuristic tool that Braudel chose to organize these “facts” will be called *exchangist*. Among the many possible approaches to the “collective des-

³⁰ Braudel, *La Méditerranée* (1st ed.), 1094.

³¹ Braudel, “Positions de l’histoire en 1950,” 30 (italics added). The last sentence, with its remarkable reiterations of organic and humanistic faiths, runs as follows: “Tout ressaisir, pour tout resituer dans le cadre général de l’histoire, pour que soit respectée, malgré les difficultés, les antinomies et les contradictions foncières, l’unité de l’histoire qui est l’unité de la vie.”

tiny" of the Mediterranean peoples, Braudel chose an economic path. And, emphasizing commercial exchanges rather than patterns of production, consumption, or distribution,³² he moved, naturally enough, to affirm the primary historical importance, "the decisive role, of [the Mediterranean] urban system. . . . [The towns] knot everything together . . . , they animate everything, they explain everything. . . ." If, for Braudel, the Mediterranean was the heart of European history, the museum of essential Man, then the cities were the heart of the Mediterranean, the pumping system that controlled the pulsation of the region's parts. "The cities are responsible . . . for a space that is heavily worked and humanized each time they set out to exploit the land and that is abandoned each time they relinquish their hold. . . . The cities are the motors of Mediterranean life, its agents, its *raison d'être*, its foyers, and the cause of its spasmodic life."³³

Braudel's solution to the problems of social or "structural" history is largely taken from the work of Henri Pirenne, the Belgian economic historian, whose name, Braudel noted, appears "at the head of the list—as is only just"—of scholars whose works are basic to the general orientation of *La Méditerranée*.³⁴ To be sure, the works of many other economic historians helped shape Braudelian social history. Braudel drew upon studies by, among others, François Simiand and Werner Sombart, who emphasized the operations of money and credit, Earl Hamilton, who provided statistical evidence on the effect of Spanish bullion on sixteenth-century prices, and Frederick C. Lane, who investigated Venetian shipping. The influence of these scholars, however, was consonant with that of Pirenne, and his work forecasts better than theirs the general substance of Braudel's exchangism.

Since Braudel held that sixteenth-century Mediterranean life played a key role in early modern European development generally, it was important for him to establish the temporal limits of the region's power vis-à-vis other areas. The problem of Mediterranean "decadence," of the dates when and circumstances in which the "curtain fell on Mediterranean grandeurs," became the main temporal problem addressed in his work.³⁵ Like Pirenne before him, Braudel looked for "structural" rather than "eventful" or "storylike" answers to this question.

³² Braudel stated his preference succinctly in a recent book, *Afterthoughts on Material Civilization and Capitalism*, trans. Patricia M. Ranum (Baltimore, 1977), 17: "Between these two worlds—that of production, where everything is made, and that of consumption, where everything is used up—the market economy served as the link, the driving force, the restricted but vital area from which flowed encouragement, energy, innovation, enterprise, new awareness, growth, and even progress. I am fond of Carl Brinkman's observation (although I do not totally agree with him) that economic history can be boiled down to the market economy, from its origin to its possible end."

³³ Braudel, *La Méditerranée* (1st ed.), 1093–94. For a similar emphasis on the decisiveness of cities, see *ibid.*, 239, 259.

³⁴ *Ibid.* (2d ed.), 2: 543. Pirenne's influence occurred quite early in the evolution of Braudel's ideas about the Mediterranean and certainly not later than 1931, when Pirenne lectured at Algiers (and Braudel was teaching in a *lycée* in Algeria at the time) "about . . . the closure of the Mediterranean after the Moslem invasions." Braudel has written of this occasion, "His lectures seemed prodigious to me: his hand opened and shut, and the entire Mediterranean was by turns free and locked in"; "Personal Testimony," 452.

³⁵ For these points, see Braudel's conclusion to the first edition; *La Méditerranée* (1st ed.), 1089, 1094, 1096, 1099. This section was radically changed in the second edition, but emphasis on the question of the moment of Mediterranean decadence was not eliminated; see *ibid.* (2d ed.), 2: 517.

The structures that mattered were those engendered by economic revival, specifically the development of long-distance trade and of urban centers with their merchant communities for the conduct of that trade. This was what had launched not only the Mediterranean area but Western Europe generally “upon the route of progress.”³⁶ Instead of viewing agrarian productivity, locally centered consumption, and a slowly broadening distribution of revenues as causes of the commercial and manufacturing activities of the cities, Braudel—following Pirenne—laid emphasis on long-distance trade, with its upper-class oriented demand structure and its mercantile concentration of surplus capital. Thus, the economy constructed by the cities was, as Pirenne wrote, “the result of an external stimulus” and not of indigenous economic efforts. Neither Braudel nor Pirenne maintained that agrarian productivity, nonluxury consumption, or the total pattern of revenue distribution was unimportant. But they made it easy to ignore the relative weight of these factors by giving primacy to the more obviously documented and quantifiable long-distance trade.

Pirenne justified his relative neglect of rural milieux and of locally rather than internationally oriented trading areas—areas where over 90 percent of the people lived, apart from a few exceptional areas in Italy and the Netherlands—by arguing backwards from the assumption of progress. Since the economy of Pirenne’s own epoch, dominated by cities, seemed to him a triumph, he deemed it important to search for the origins of this happy development; and, not surprisingly, he turned in this search toward the history of those same great cities and their early exchange activities. They, not “agricultural civilization,” lifted Mediterranean and European space out of an otherwise relentless, almost immobile repetition.³⁷ Preoccupation with this same set of problems, growth and progress on the one hand and the decay and even “death” of the Mediterranean economy on the other, finally also set the terms within which Braudel wrote of what happened around the Interior Sea during the time of Philip II.

The Mediterranean is a “person.” Just as its growth could be explained by reference to the area’s urban “heart,” its decay was analyzed in terms of “perturbations of circulation.” Difficulties in the exchange system, Braudel con-

³⁶ For Venice’s contact with Byzantium and the Flemish coastal cities’ contact with the “Russo-Scandinavian world,” from which the eleventh-century commercial revival received its impetus, see Pirenne, *Medieval Cities*, trans. F. Halsey (Garden City, N.Y., 1956), 72–74, 58–62.

³⁷ Pirenne, *Medieval Cities*, 53, 58. Also see his *Mohammed and Charlemagne*, trans. B. Mial (New York, 1957), 236. In a later work, Braudel paid greater attention to interaction of a city-centered trade with rural areas than he had in *La Méditerranée* but primarily in order to emphasize, even more than he had earlier, the world-historical importance of the cities’ “capture” and subjection of village markets to urban control; Braudel, *Civilisation matérielle, économie, et capitalisme, XV^e–XVIII^e siècle*, 3 vols. (Paris, 1967–79), esp. 2: 42, 414, 535. The interlocking connection of a version of economic theory that gives primacy to exchange, a version of social theory that gives primacy to cities as the centering locus of human activity, and progressivist theory of European history is even more in evidence in this new work than in *La Méditerranée*. Cities, to Braudel, have always stood at the center of history’s “world economies”; *Civilisation matérielle*, 3: 16, 22. Four such cities in Italy—Florence, Genoa, Milan, and Venice—constructed the Mediterranean “world-economy”; *ibid.*, 12. For a more general discussion of this work, see note 65 pages 91–92, below. Braudel expressed a much more relativistic position than this twenty-six years earlier, shortly after publishing the first edition of *La Méditerranée*. In a debate with Labrousse, Braudel maintained that it is wrong to argue backwards in time from nineteenth-century urban domination of the countryside. Sometimes the cities “swallow” rural markets, but at other times rural networks of exchange swallow those of the cities, dissolving the economic force of the latter. For this debate, see Georges Friedmann, ed., *Villes et campagnes: Civilisation urbaine et civilisation rurale en France* (Paris, 1953), 26–30.

cluded, and not primarily in productivity, distribution, or consumption, led to a “structural crisis” and thence to the end of Mediterranean “splendors” in the early seventeenth century.³⁸ Braudel’s formulation of the crisis adapts to the sixteenth and seventeenth centuries Pirenne’s theory of the decline of Mediterranean trade in the early Middle Ages. In *Mohammed and Charlemagne*, Pirenne argued that the barbarian invasions (ca. 250–600), however great their effect in destroying the Roman *political* unity of the Mediterranean, did not destroy the region’s *economic* unity; only the spread of Islam in the seventh and eighth centuries accomplished economic disintegration. And, Pirenne claimed, in general historical terms the economic unity that was maintained during the third, fourth, and fifth centuries was more important for the tenor of everyday life than was the dissolution of the Roman Empire. In *La Méditerranée*, Braudel has similarly argued that Spanish-Portuguese overseas expansion, however significant their naval discoveries were in the political construction of the two great Iberian empires in the sixteenth century, did not immediately disrupt Mediterranean economic life; only when the new network of transatlantic and north-south routes had matured did the “center” of economic “life” transfer itself elsewhere. And, Braudel claimed, Mediterranean “glory,” dependent on old pathways and customs of exchange that were slow to change, did not, therefore, fade until the seventeenth century.

THE CONCEPTUAL NOVELTY of *La Méditerranée* lies not in Braudel’s “structural history” but in his geohistory. Because of, not in spite of, his ideological presuppositions, Braudel moved in the geohistorical section of his book in heuristically novel ways that yielded rich experimental results. The new heuristics gave historical form to nonhuman aspects of social life, an achievement that has taken on greater meaning as consciousness of our ecological dependencies increases.

Those who had emphasized the historical role of climate, soil, and water before Braudel—a discontinuously related group of writers from Polybius, Bodin, Montesquieu, and Buckle to Paul Vidal de la Blache and Lucien Febvre—had not succeeded in finding a way of interlacing the effects of such ecological considerations with social activity in temporally specific ways. Historians had generally introduced their subject by a chapter that treated geography and demography as background. Once the stage had been draped with a physical space and an apportionment of its rural and urban, upper- and lower-class settlements, narrative could begin; indeed, in 1912, Febvre introduced his doctoral thesis on the Franche-Comté of Philip II’s time in this manner. By 1922, however, Febvre had revised his work to set forth programmatically a more continuous interweaving of geography and history than he had done just a decade earlier. In the first edition of *La Méditerranée*, Braudel implemented the program that Febvre had envisioned.³⁹

³⁸ Braudel, *La Méditerranée* (1st ed.), 1095, 1097.

³⁹ See Febvre, *Philippe II et la Franche-Comté*, and *La Terre et l’évolution humaine*. Febvre’s main articles on the relationship between geography and history are collected in his *Pour une histoire à part entière* (Paris, 1962).

Braudel's choice of a subtitle for Part 1 of the first edition, "The Share of the Milieu," indicates his strategy for joining geography and history: he wanted to depict how human and nonhuman elements mix to produce the Mediterranean "milieu," not merely its "environment." The complex of physical media by which, together with which, and struggling against which men and women carry on their activities takes a place at center stage, playing its role as a "grand . . . historical personage."⁴⁰ These physical media no longer constitute a backdrop for human activities, a surrounding environment that opposes itself to social life as Nature to Culture. *La Méditerranée* shows mountains and plains not only as vehicles for but also as progenitors of population changes; seas and coastlines mold, and are molded by, the economic and imperial ambitions of cities; wind and rainfall patterns thwart, and are thwarted by, the inventiveness of merchant sailors trading at ports along the Mediterranean shores.

Braudel used two heuristic modes to investigate and display the interworking of physical and biological conditions with social and economic endeavors. Both modes employ concepts of rhythm, of to-and-fro, or harmonically repeated, activity. Shepherd, merchant, and sailor moved their hands, pulled in different ways upon the ropes of society, and cooperated in ways whose pattern was unknown or obscure to them personally, scattered as their motions were over time and space. These motions joined each other to form rhythms of material force that eroded hillsides, constructed marketplaces, and crowded the holds of ships with precious cloth. The interaction of the configurations of land- and seascape, winds and tides, sea-going technology, grazing patterns of livestock, market forces, and the training of men to perform diverse tasks such as sailing, shepherding, or merchandizing is scarcely decipherable unless each aspect of the process can be given form by studying it separately, stretched out upon a large-scale frame of space involving the whole Mediterranean area and its hinterlands. This is Braudel's first heuristic mode: tracing how one kind of activity led to another in ways that were quite beyond the consciousness of any particular group of shepherds, of any particular combination of merchants, or even of any specific diplomatic service, however ingenious its intelligence network, Braudel showed that such diverse activities eventually knitted together to form man's interwoven, "concrete reality." By pulling apart and examining separately the diverse activities of city merchant or village farmer, by following the different paths of the wool, wheat, or wine trade, by studying the total network involved in the raising, grazing, and marketing of sheep, Braudel was able to demonstrate how a given action spread outward, combining now with this and now with that to cause ever-enlarging ripples of change, as a pebble tossed into water creates ever-widening circles of waves.

The purpose of Braudel's second heuristic procedure is less to distinguish the paths taken by individual economic activities or social enterprises than to in-

⁴⁰ The complex of physical media is, of course, the Mediterranean Sea; see Braudel, *La Méditerranée* (1st ed.), 13. The idea of "milieu" as an active historical force had been developed in French historiography by Hippolyte Taine, although Taine used the term to combine geographical with psychological no less than social factors. Taine's use of "milieu" did not, therefore, constitute a direct model for Vidal or Braudel. See Sholom Kahn, *Science and Aesthetic Judgment: A Study of Taine's Critical Method* (New York, 1953), 104.

dicating how diverse paths may nevertheless run parallel while being widely separated in time and space. Here, as in his first heuristic procedure, Braudel needed a large-scale frame of reference. Instead of moving from one part of the Mediterranean to another, as if meandering leisurely along country roads to examine each region's peculiarities, Braudel sought a vantage point on high from which he could see how the rivers connect to form systems flowing to the seas, how peninsulas, mountains, and deserts form repetitive series, configuring the landscape into a totality composed of geographic types rather than of regionally disparate entities. In regional differentiation, things have their special names and their unique places. But in Braudel's regional unification, rivers, hills, and forests of a certain type in one part emerge again with different names in other parts: Mediterranean continuities, not the differences between the Arno and Po waterways or between Alpine and Pyrenean ecology and demography, become critical. In both heuristic modes, therefore, Braudel's geohistory relates the systemic qualities of natural forces to those of social forces such that the two are seen to evolve in relation to each other.⁴¹

Braudel used the expansively disarticulatory method in combination with the systematically interweaving approach throughout *La Méditerranée*. The most obvious but not the only examples occur in Part 1. For instance, Braudel showed how land and sea routes outside the Mediterranean area conditioned the routes inside the region from the Middle Ages through the sixteenth century. The question was not simply how Saharan trails, Alpine passes, or Atlantic paths from Spain to the New World were related to the location of Mediterranean cities, harbors, or inland highways. The system and placement of routes on land and sea determined, and was determined by, the type and volume of merchandise carried along them. The nature and amount of goods traded were, in turn, conditioned by where and for whose satisfaction they were produced. The medieval pepper trade with the East, for example, established a system of routes that was partially dislocated in the fifteenth and sixteenth centuries by the Portuguese discovery of the Atlantic route around the Cape of Good Hope. But the medieval route through the Red Sea, which so benefited Venice and other Mediterranean cities, re-emerged as the primary one between 1550 and 1600, because it represented a social, no less than a natural, system, connecting a long chain of intermediate economic interests that were not served by the African route. Strongly organized merchant societies scattered along the Indian Ocean-Red Sea route from India to Egypt, accustomed to trading such traditional articles as coral, saffron, opium, and mercury, necessarily seized every opportunity

⁴¹ Febvre's *La Terre et l'évolution humaine* programmatically advocates systemic understanding of the interaction of nature and society; it thus marks a theoretical break with his earlier procedures in *Philippe II et la Franche-Comté*. For the study of the Franche-Comté, Febvre used the regionally particularizing style of treating ecological material in the first chapter of his study; hence, the natural and the social, the geographical and the historical, remain polarized. Chapter 1 stands as a geographically introductory background to the foreground of history. The frontiers of a state, moreover, give Febvre's Franche-Comté unity, unlike Braudel's Mediterranean area, whose unity is socio-ecological. Regions formed primarily by political acts also mark the great regional histories by Pierre Goubert, Pierre Vilar, and Emmanuel Le Roy Ladurie, which in other respects exemplify many of Braudel's procedures. Regional geohistorical syntheses are, in any case, not a creation of French historians. Many earlier examples are found in the German tradition of *Landesgeschichte*, dating from such works as those by Justus Möser in the eighteenth century.

to undo the Portuguese initiative. These local merchants eventually won because maintaining a route required protection; the Portuguese could not at every point and at all times exercise the political and military control necessary to preserve their monopoly by closing the Red Sea.⁴² Thus, Braudel simultaneously pulled together the spatial elements of the pepper trade over a vast area and distinguished the geoeconomic and geopolitical components of commercial change that sometimes worked together and sometimes played at cross purposes.

Therefore, in spite of the deterministic thrust of his ideological precepts, the details of Braudel's analyses often evince an open-ended, relational disposition of historical forces. The two heuristic procedures lead to the display of spatial patterns whose parts act and react on each other in ways that cannot be totalized, because new associations—subparts and superior parts—constantly form and dissolve. Since the elements of these spatial patterns are physically and sensorially definable, they have a resemblance to the sense of the word "structure" envisioned by Roupnel and adopted by Braudel. Yet, because that word is double-edged, Braudel could respond to the vogue of social-scientific structuralism in such a way as to attribute fundamental importance to the concept in the second edition while nevertheless expressing distaste for its employment by most of his contemporaries.

HOW CAN BRAUDEL'S AMBIVALENT STATEMENTS about structural method at the end of the second edition be explained? "I am a 'structuralist' by temperament. . . . But the 'structuralism' of a historian has nothing to do with the set of problems, going by the same name, that torment the other human sciences. It does not direct him toward the mathematical abstraction of relations, which are expressed as functions, but toward the very sources of life, toward what is most concrete, most everyday, most indestructible, and most anonymously human about it."⁴³ Of the two structuralisms competing with each other in the "human" sciences, then, one remains in contact with perceptible features of life, while the other abandons such contact. Braudel's differentiation between these two meanings of structure can be related to a more general distinction, common in the social and natural sciences since the sixteenth century, between apparent framework and internal system or between surface pattern and generating law. The same word, "structure," has been used, however confusingly for the reader, to express both the first and the second members of these pairs. When "structure" has been employed to refer to something physically and sensorially experienced (apparent framework), it has more frequently occurred in organic contexts (for example, a body's "structure" is its skeletal form). When "structure" has been employed to refer to something implied by a sensory presence (gener-

⁴² Braudel, *La Méditerranée* (1st ed.), 421–34.

⁴³ *Ibid.*, (2d ed.), 2: 520: "Mais le 'structuralisme' d'un historien n'a rien à voir avec la problématique qui tourmente, sous le même nom, les autres sciences de l'homme. Il ne le dirige pas vers l'abstraction mathématique des rapports qui s'expriment en fonctions. Mais vers les sources mêmes de la vie, dans ce qu'elle a de plus concret, de plus quotidien, de plus indestructible, de plus anonymement humain."

ating law), it has more frequently occurred in mechanistic contexts of science.⁴⁴ Not the body's anatomy but the pre-organic or generally material systems giving rise to anatomy—the principles of body chemistry, of blood flow, of glandular secretions that generate skeletal form—constitute “structure” in this sense. Both meanings of structure concern the general rather than the local and particular, but the second sense refers to the set of principles implied by the form of a thing. Yet it is this form or general pattern exhibited by a thing to which the first sense refers. Henceforth, the word “structure” will refer only to the second, implicatory sense; the word “pattern” will provide a substitute for the first, sensorially referential sense of structure.

In the social sciences “structure” was predominantly used in the sense of pattern until the 1950s. In France after 1950, structuralists such as Claude Lévi-Strauss combatted this usage. In a well-known article of 1953, he related the two senses of “structure”—pattern and structure—to the methodological distinction between the “empirical” observation of social relations and the analysis of those relations. “The term ‘social structure’ has nothing to do with empirical reality” but with “models which are built up after it,” he wrote. “Social relations consist of the raw materials out of which the models making up the social structure are built, while social structure can by no means be reduced to the ensemble of the social relations to be described in a given society.” The structure of a society is, according to Lévi-Strauss, an implied entity discernable only after social relations have been ordered as an ensemble or pattern in the sense proposed here.⁴⁵

Braudel taught side-by-side with Lévi-Strauss in the École Pratique des Hautes Études during the 1950s and 1960s while revising *La Méditerranée*. Moreover, Braudel was strongly attracted by the contemporaneous movement toward a formal and abstract—that is, a structural rather than only a patterning—approach to economics and economic history developed by other teachers at the École, including François Perroux, André Marchal, Jean Lhomme, and Jean Weiller,⁴⁶ although the very rigor of much of economic structuralism repelled him because it seemed to set such inquiry at too great a distance from the

⁴⁴ These distinctions represent tendencies only. Equating the opposition organic-mechanistic with the opposition physically present-logically implied is as misleading as it is to suggest, with Braudel, that research procedures that are life-oriented (organic) are necessarily opposed to those that are aimed at quantifying behavior mathematically (mechanistic) in the same way that the concrete is opposed to the abstract.

⁴⁵ Lévi-Strauss, “Social Structure,” in A. L. Kroeber, ed., *Anthropology Today* (Chicago, 1953), reprinted in Lévi-Strauss, *Structural Anthropology* (Garden City, N.Y., 1967), 271.

⁴⁶ On these men's methods and the development of structural method in economics in general, see Gilles-Gaston Granger, *Méthodologie économique* (Paris, 1955); Émile Levy, *Analyse structurale et méthodologie économique* (Paris, 1960); Peter McClelland, *Causal Explanation and Model Building in History, Economics, and the New Economic History* (Ithaca, N.Y., 1975); and Jean Viet, *Les Méthodes structuralistes dans les sciences sociales* (Paris, 1965). In 1950 Braudel, Lhomme, and Weiller became the founding editors of a new periodical, the *Revue économique*, that aimed to foster methodological exchanges between economists and historians. Braudel devoted one of his seminars at the Collège de France during 1952–53 to economic method, specifically discussing the structural economics of Jean Lhomme and Johan Akerman and studying the reports of the Congress of French Economists of 1952, which were devoted to “the emergence of a theory of structures” in economic science. For an indication of the themes of this congress, see René Clemens, “Prolégomènes d'une théorie de la structure économique,” *Revue d'économie politique*, 62 (1952): 971–1001; and, for a description of Braudel's course, see the *Annuaire du Collège de France*, 53^e année (Paris, 1953), 251–52.

documentable behavior of men. But this rejection of social-scientific structuralism in the name of concreteness and fidelity to empirical reality was based on a misapprehension, for the idea of structure remains concrete in spite of the abstractness of its expression. There can be no structure without the sensorially identifiable pattern of which it is the implication. Conversely, there can be no pattern, large or small, of long or short duration, that does not give evidence of the working of systemic laws or structures. When structural analysis is understood as a heuristic procedure that probes the modes of organization to which different patterns belong, it can no longer be thought to lead to useless abstraction. And, in any case, in social-scientific practice the difference between observable patterns and implied structures is relative rather than absolute; to create a consistent schema and terminology, therefore, always requires great care.⁴⁷

Why, then, does theorizing about structures inspire such distrust? Because Braudel's opposition between concrete and abstract is related to his differentiation between down-to-earth and vague history, between history tied to the soil and history that speaks airily, and, more generally, between "material life" and life's ideal aspects, "life" must be concrete. Its concreteness in Braudelian rhetoric insures its enduring quality. Historical research that remains oriented toward the concrete will not float away, disappearing into the fogs of abstraction and losing apprehension of the material bases of life. The historian's concern with concreteness will, therefore, stimulate awareness of life's "anonymous" "indestructibility" beneath a distractingly changeable surface. To have accepted the structuralist notion of structure—with its implied, recondite, and often mathematically abstract character—would, in fact, have drawn into question Braudel's organic-humanistic understanding of human affairs. In his revisions of 1966, therefore, he was confronted with a double, almost contradictory task. Given the *Annaliste* commitment to a historiography synthesizing the social sciences, some account needed to be given of the mounting vogue of structuralism, to which Braudel was in any case indebted for certain tools of economic analy-

⁴⁷ In analyzing even a single phenomenon, such as the production of a bolt of cloth, let alone a complex phenomenon such as the production of all woolen cloth in some sixteenth-century European nation, a historian may well decide to prepare a series of schemas ranging from those that are patterns (which employ terms refining very little upon the terms found in the primary documents) to schemas that are half-patterning and half-structural in quality (in that they are somewhat implicatory rather than directly referential to the documents) and to those that are structures; a historian may even proceed to elaborate, beyond a first level of structurality, systems of structures that are quite general in their terminology and might be called "implications of implications." The possibility of moving to several different levels of abstraction in using the concept of structure can be illustrated by reference to an early work of the economist François Perroux, who advocated a structural approach to economic analysis in his university courses in Paris in the 1930s and defined "structure" as the set of logical implications of an observable pattern—in this instance, "a concrete capitalism": "Par 'structure' on entendra les *proportions* et les *relations* qui caractérisent un capitalisme concret ou, plus généralement, un ensemble économique localisé dans le temps et dans l'espace." And Perroux specified the variety in scope of different possible structural analyses: "Il est possible de l'utiliser pour rendre compte d'ensembles économiques beaucoup plus petits que la nation. Ainsi on peut faire l'étude structurale de l'entreprise." But Perroux also referred to several sets of more general economic ensembles, using the term "system" in a manner similar to that in the phrases "systems of structures" and "systems of systems": "Chaque fait, chaque événement économique doit être situé non seulement dans un *système* (économie fermée, artisanat, capitalisme), mais encore dans une *structure* déterminée de ce système. L'économie fermée a des structures bien différentes selon qu'il s'agit de l'économie de famille, de tribu, de clan, ou de village. L'économie artisanale offre d'innombrables *diversités structurales* (artisanat agricole ou urbain ...)." Perroux, "Pour un approfondissement de la notion de structure," in *Mélanges économiques et sociaux offerts à Émile Witteur* (Paris, 1939), 271–72.

sis. At the same time, he had to ward off danger to the system of assumptions interwoven not only into *La Méditerranée* but into his understanding of the *Annales* enterprise generally.⁴⁸

Braudel's major move in attacking these problems in the second edition was to jettison "the structural history of Gaston Roupnel." References to Roupnel's organic invocations of structurality were deleted, and Part 2 was no longer called "structural history" in contraposition to Part 1's "geohistory." Part 2 of the second edition "mixes" structural with nonstructural—or "conjunctural"—history, by making use of the economists' "true distinction" between structure and conjuncture. In the introduction to Part 2, Braudel phrased this distinction as that between "the immobile and the mobile, slowness and excessive speed." In the first edition, he had given immobile, slow history the name "geohistory"; in the second, he called it "structural history."⁴⁹ The shift in the terms describing the book's parts implies a considerable shift in categorization: Braudel could pursue "structural" history in any section of the work dealing with "immobilities"; geohistory and social history no longer served as fundamental classes of historical action. Immobilities of a geographical sort, studied in Part 1, were now considered merely one instance of structurality among many; economic immobilities formed another class of structures whose study, along with that of economic "conjunctures," swelled the pages of Part 2, augmenting its volume by nearly one-third between 1949 and 1966. While striving to accommodate his concept of structure to the then-prevalent—that is, logically implicatory—tradition of defining the word, what Braudel called "structure" in the second edition remains in all essentials attuned to the sensorially referential way of defining the word. From the point of view of twentieth-century structuralism, Braudel's structures are long-enduring patterns, associated groups of activities that change their mutual relations but slowly.

⁴⁸ At first glance, there seems to be inconsistency in a rhetoric that associates "depth" not only with greater substance and material being but also with greater concreteness than is attributed to "surfaces," which are at least sensorially more immediate to eye, ear, and touch. The apparent inconsistency is, nevertheless, a traditional part of organic-humanistic assumptions, which insist on the primacy of interior or underlying reality, on the "heart" rather than on the limbs, and yet on outward responsiveness, sensibility, and lively perception more than on indrawn conception and abstract thought. A more serious question of consistency, however, is involved here: how did Braudel, combatting an abstract structuralism, conciliate his search for "the very sources of life" in what is "most *anonymously* human," with his empiricist insistence on the historian's duty to investigate human activity in all of its differentiated forms? Humanistic and empirical aspects of Braudel's statements about method clash sufficiently to indicate the vanity of supposing that any historian or social scientist can do without either abstract thought or concrete example. On the one hand, the humanistic tradition (with its insistence on basic human verities), if pursued singly, leads historians to presuppose a "nature" interior to man as the solid ground of human behavior; the historian's work, however "concrete" in language or example, will then recover chiefly those portions of the past that accord with the preconceived limits of this human nature. On the other, the empirical tradition (with its insistence on affirming only what can be found in archives), if pursued singly, leads historians to paraphrase sources and, hence, unreflectingly to reproduce the ideological assumptions of those sources, no matter how shallow, time-serving, and contradictory they may be. But the object of historical research, if it is to be coherently displayed in its connections with other associated objects in both past and present time, cannot be found empirically or recovered humanistically. It can only be constructed, which means that each search in the archives for the concrete is—and must be—accompanied by attempts to categorize abstractly what is found there. In the practice of many historians, however, this construction is implicit rather than explicit, unconsciously rather than consciously pursued; and, therefore, the intertwining of abstracting and concretizing procedures in much historical work is ignored.

⁴⁹ Compare Braudel, *La Méditerranée* (1st ed.), xiii and 307, with *ibid.* (2d ed.), 1: 16 and 325.

Consider, for example, the “structure” that, Braudel claimed, controlled routes and the flow of traffic. An increase in commerce during the sixteenth century caused the use of land and sea routes in the Mediterranean area to increase in the same degree: “a certain equilibrium [in the growth of the two kinds of routes] seems to be maintained.” As an example of this proportional growth, he cited the rivalry between land and sea routes for shipping wool from Spain to Venice between 1588 and 1606. Although the sea route was preferable because of the virtually free entry granted in 1598 to wool arriving by sea and also because of the introduction of newer, faster Dutch merchant ships in the 1590s, the overland route via Genoa or Livorno continued to be more favored by the shippers, just as it was before 1598. Having offered several possible reasons for this stable relationship, Braudel concluded, “Here is a good example . . . of the inelasticity of competition between the two kinds of routes and of the complexity of the factors involved. But a structural permanence of relations [*rapports*] can be guessed at and retained as a hypothesis for research.” Thus Braudel deftly joined reference to some kind of structural logic regulating the relation between the two types of routes to an emphasis on the endurance of the relationship.

Braudel then cited a second relationship between land and sea routes that introduced a “microscopic observation” lacking in the first example. Comparing customs records of Castile’s ports with those of its frontiers with Navarre, Aragon, Valencia, and Portugal, he found that taxes collected at entry and exit points on land routes in 1560 were one-third as high as those on sea routes; in 1598 customs duties still stood in a ratio of one to three. Braudel therefore surmised,

Structurally, nothing apparently changed in the relationship between Castilian routes in the second half of the sixteenth century. Everything progressed at the same rhythm or along the same curve, to speak in the language of our graphs. Here, then, is something that shows with greater precision than our last examples a certain equilibrium of the two kinds of routes with respect to each other: their totality [*ensemble*] maintained itself in proportions [*rapports*] that varied little.

The curves that on a graph represent the augmenting volumes of the two kinds of traffic move in the same direction and in the same proportion; the “structured” relation of the two is thus observed to last for at least forty years. Braudel closed his inquiry with this demonstration that the relationship endured.⁵⁰ He did not try to account for the relationship. Yet, in the absence of any analytic attempt at explanation, what reason is there to surmise that a forty-year-long parallelism of Castilian customs duties (and, thus, supposedly of trade on land as opposed to sea), however precise it was, has predictive value “as a hypothesis for research” in other parts of the Mediterranean at other times? Braudel noted, for example, that sixteenth-century tax figures were regularly manipulated by the Castilian bureaucracy for reasons of policy. Is the precision of his structured relation, then, caused by systems of government bureaucracy, by those of economic life, or by the interaction of both?

⁵⁰ Braudel, *La Méditerranée* (2d ed.), 1: 266, 268, 269, 270.

The distinction between pattern and structure is implicit, but unexplored. There is a pattern of sea and land trade through Castile from 1560 to 1598, and there is another pattern of sea and land trade between Spain and Venice from 1588 to 1606. But the system of transformable relations implied by the two patterns—a structured complex of commercial transportation that takes into account political, economic, and other variables—is not offered.⁵¹ A structural model of social activity shows how an ensemble of relations is interconnected, so that a set of factors like that involved in commercial transport can be seen to be mutually conditioning. Each of the factors in a structured system is itself a system, and thus the structural ensemble is a system of systems, a generalized form of the logics at work in each separate system. In Braudel's example, the systems of government fiscality that involve customs duties and thus differentially restrict or facilitate commodity movements need to be related to the market systems that stimulate or discourage transportation of goods and to the systems of transport that relate technological means of transferring goods to variables of geography and climate. In this way, a system of systems, a "mechanism" of commercial transport, might be discovered. The "structural permanence of relations" that Braudel "guessed at" contributes little to this goal. The alleged equilibrium between routes is a fragment of a possible structure. It offers one piece of evidence about a relation among systems unaccompanied by other evidence or by analysis of the way the systems concerned may generate such an equilibrium.⁵²

⁵¹ "Transformable relations": If the production of cotton goods in England today is compared with textile production a century ago, the patterns of production would emerge as different: different materials, different tools, different ways of working with the materials and tools, different inputs of capital, and so on. But a comparison of the models of the two patterns to the model of the structure of capitalist production would probably reveal similarity in the way the materials, tools, labor, and capital are combined. If the relations among the factors of production in the two systems are shown to have the same productive structure, the two are said to be transformable. As Lévi-Strauss put it, "For any given model there should be a possibility of ordering a series of transformations resulting in a group of models of the same type"; *Structural Anthropology*, 271–72. The tools, input of capital, labor skills, and so on required in the production of cotton cloth in France in the 1970s, in England in the 1970s, and in England in the 1870s would probably prove to be "transforms" of the same structural relations. But those required by textile production in England in the later sixteenth century would almost certainly not be transformable into the system of the 1970s.

⁵² Charles Tilly has commented on the disinclination of French historians to pursue patterning analysis, let alone structural analysis, far enough to substantiate historical hypotheses of any scope and soundness. Tilly cited examples from recent French historiography, including some of Braudel's work, of three kinds of patterning procedures and of two others more structural in thrust, all of which have in common the insufficient use of quantification to pinpoint and verify hypotheses. With reference to the "systematic quantitative study of group differences" (one of the patterning procedures), Tilly concluded, citing an example similar to that on Castilian trade in Braudel's text, "Although most French scholars are aware, for instance, of the general correspondence among the distributions of modern industry, transportation lines, and literacy, I have not found a single statistical study of the extent and form of their interdependence." Tilly also commented on the method of "correlations," which moves toward structural conclusions, and noted, "Most efforts to deal with such problems [as correlating political party adhesion with holding property] have been through the crude and deceptive procedure of comparing maps representing the distributions of the two phenomena in question—comparing them by eye, at that. . . . This is a pity. Much historical argument is relational: A goes with B, A causes B, A and B jointly produce C. A good many relational arguments would benefit from being brought out into the open and subjected to quantitative verification." And, as Tilly further pointed out, they would also benefit from being subjected to a logical analysis of the consistency of the "links" or "sets of relations" said to "persist" among the two, three, or more historical variables being correlated. Tilly, "Quantification in History as Seen from France," in Val R. Lorwin and Jacob M. Price, eds., *The Dimensions of the Past* (New Haven, 1972), 103, 107, 106. I have added the categories "pattern" and "structure" to Tilly's comments; he used instead such terms as "trends" and "group composition" for the first category and "sets of relations" for the second.

None of the structural systems studied thus far has proved to be universal in human societies; conversely, none has been shown to be implied by only one temporally and spatially limited social pattern.⁵³ Structural method thus lends little support either to the historicist dictum that each social group develops a unique historical life incompatible with all others or to the scientific affirmation that social behavior is reducible to universally applicable laws. Those who, in the 1950s and 1960s, argued that structural and historical methods were intrinsically incompatible presupposed for the most part these eighteenth-century doctrines of science (Newtonian universalism) and history (Herderian uniqueness). In fact, historical inquiry cannot be confined to establishing the uniqueness of events or national traditions; systemic questions inevitably arise even in such traditionally delimited research. Nor can structuralists ignore the historically contingent evidence upon which they must draw for their generalizing, relational conclusions.⁵⁴

BRAUDEL CAME CLOSEST TO INITIATING THE HISTORICAL USE of structural method in the very area that he had distinguished as nonstructural in the first edition—that of geohistory. There he was able to draw upon sciences—geology, meteorology, and biology, most notably—that had worked out some of the systems of relations implied by geohistorical patterns. The pattern of a geographical area depends on a particular collocation or structural relation among the logics that these sciences endeavor to specify. To carry out patterning and structural analysis in all details for a historical project of any scope would be difficult; if the historian's primary purpose is a description of the evolution of patterns across wide spaces and through long time periods, great explicitness about the systems and subsystems involved at one time and place becomes narratively superfluous. Thus, Braudel sketched a geohistorical structure and pattern in a few lines in a section entitled "Drought: The Explanation of the Mediterranean."

The great disadvantage of this climate for human life lies in the annual distribution of rainfall. It rains a good deal, even far too much in some places. But the rains come in autumn, winter, and spring, principally in autumn and spring. It is roughly the opposite of a monsoon climate. The monsoon climate arranges a fruitful meeting between heat and water. The Mediterranean climate separates these important factors of life, with predictable consequences. The "glorious skies" from late spring to mid-fall have their severe drawbacks.⁵⁵

⁵³The less than fully particular or unique yet less than fully universal character of structural systems is equally applicable to the kinship structures of contemporary tribal societies that Lévi-Strauss and others have defined, to the industrial-capitalist structures of production and exchange that economists are delineating, to the tripartite system of political institutions characteristic of early Indo-European societies that Georges Dumézil has described, to the Indo-European phonological structures that Roman Jakobson, Nicolai Trubetzkoy, and other linguists have worked out, and to the semantic codes in European literature, science, and art that such scholars as Roland Barthes, Umberto Eco, Michel Foucault, and Tzvetan Todorov have found.

⁵⁴But structuralists do methodologically ignore the historicity of their evidence about as often as historians ignore structurality. Ideological blinders are just as opaque on both sides of the methodological divide. Hence, the polemics.

⁵⁵Braudel, *La Méditerranée* (1st ed.), 203. Braudel altered the title, but not the text, of this subsection by 1966; *ibid.* (2d ed.) 1: 218.

Abstracting from this description to draw out its analytic features, the passage might be phrased thus:

Meteorological and biological systems interact so that some rhythms in the possible combinations of high temperature and high rainfall produce abundant crops while others produce little. (The geological character of the soil—calcareous or heavy with clay, for example—also affects food production; Braudel omitted this factor, perhaps as a local variable.) The rain-plus-heat (and light) rhythm, occurring from late spring through summer to mid-fall, produces high yields. The rain-minus-heat (and light) rhythm, occurring from mid-fall through winter to late spring, produces low yields. The “glorious skies” of a Mediterranean summer accordingly signal a pattern of low food production, while the heavy clouds and regular summer rains of central India signal a pattern of high food production: these patterns represent two different forms—two transforms—of the same system of relations.

Braudel did not use the word “structure” in this section, although his statements depend on an understanding of the relationship between meteorological and biological factors in a structural way. He aimed his narration not at this structure but at its “concrete” results, at the pattern and the results of the pattern in terms of human actions and reactions.⁵⁶ He went on to emphasize that dry farming and irrigation have been used in Mediterranean lands since at least the sixth century B.C. to modify or entirely thwart the effects of this rhythm. In Braudel’s terms, this long-enduring pattern of interaction among dry farming, irrigation, climate, and crops would presumably constitute a “structure”—a pattern of food production implying systems regulated not only by meteorological, geological, and biological factors but also by technological, marketing, political, and cultural factors that mentally and physically control agriculturalists’ efforts. A structural analysis that reveals the interlacing of all of these systems almost inevitably stimulates reflexive perception of more patterns and subpatterns that affect the situations under study than were first supposed.

Any historical situation consists of a system of patterns that implies a system of structures. Viewed as a system of patterns, a historical situation has a shape or configuration, usually depicted in structural analysis as a polarity between harmony and conflict. The more conflict or tension among the patterns making up the configuration, the greater the tendency for one or several patterns to change.

⁵⁶ The unintended consequence of this emphasis on human actions and reactions is that in historiographical practice Braudel, who has championed the reconciliation of history with other social sciences, has actually widened the cleft between them by treating the difference between patterning and structuring approaches as a choice between good and bad methods. He has criticized as unsuited to historical inquiry what are, in effect, the structural methods of geographers and biologists, because these methods have seemed to him too rigid in their positing of scientific limits: “*Le danger même serait de vouloir mettre en cause un domaine délimité une fois pour toutes. Que géologues, botanistes, géographes, et biogéographes procèdent ainsi, c’est normal: plus ou moins grand, leur territoire est toujours borné avec précision; ils peuvent le clôturer de poteaux scientifiques, légitimement plantés. La ligne des Pyrénées, des Alpes, les montagnes de Crimée et du Caucase, puis au Sud, le rebord de la grande plateforme rigide afro-asiatique, telles sont les bornes admises par les géologues.*” *La Méditerranée* (1st ed.), 139 (italics added).

If structuralists proceed historically, as they should, then they will perforce include not only discussion of the relations between one pattern or structure and others but also configurational analysis of the ways in which patterns on one level and structures on another harmonize or conflict among themselves at a certain time and place. Patterning, structuring, and configurational approaches would seem to be equally indispensable to that "global history" of the Mediterranean at which Braudel aimed.

Although Braudel's economic history is largely patterning in method and purpose, one section of Part 2 in the second edition introduces, like some portions of his geohistory, a new structural approach.⁵⁷ In a subchapter entitled "A Model of the Mediterranean Economy," Braudel applied the technique of national accounting, particularly associated with the work of Jan Marczewski in France and Wassily Leontief in the United States. Braudel's enterprise is audacious, for he applied this input-output model—developed for modern, nationally enclosed, and statistically elaborated areas—to the premodern, multinational, and statistically undefined Mediterranean area. Braudel has made tentative, approximate quantifications of supply, demand, production, and consumption for the sixteenth-century Mediterranean area and justified the speculative quality of the material he had to introduce to give cohesion to the model by concluding, "These theoretical figures are not absurd, nor are they useless by any means. We have made a sketch, [and] this was necessary so that we could, as it were, situate great inaccessible masses of landscape [*paysage*] in relation to one another [*les uns par rapport aux autres*]." ⁵⁸

Braudel developed this economic model in terms that do not include the word "structure," just as he omitted the word from passages in which he offered brief structural analyses of ecological systems. Paradoxically, the two initiatives toward structural rather than patterning history in *La Méditerranée* are carried out without reference to structurality, while his predominantly patterning practice is repeatedly called "structural."⁵⁹ Braudel's invocation of the term thus appears to be intentionally ambiguous, floating between its sensorially referential and logically implicatory meanings so as to accommodate a methodological syncretism, making use of "all of the human sciences," within the limits of organic

⁵⁷ Braudel emphasized the path-breaking quality of his "structuralism": in the "immense domain of structural history," he concluded in 1966, as he had in 1949, that nothing exists except "a few works of prospectors"; *La Méditerranée* (1st ed.), 1126, and *ibid.* (2d ed.), 2: 544.

⁵⁸ See Braudel, *La Méditerranée* (2d ed.), 1: 419, 383–419. For a brief explanation of national accounting, see McClelland, *Causal Explanation and Model Building in History*, 180–84. Braudel's use of national accounting has been attacked for its incorrect application of the model's logic and method of calculation as well as for its incomplete documentation, so sparse as to be useless; see Jan de Vries, "The Classics in Transition," *Reviews in European History*, 1 (1975): 473.

⁵⁹ The words "structure" and "structural" occur eleven times in the first edition, six of which are in prefatory, concluding, and bibliographic matter; Braudel, *La Méditerranée* (1st ed.), xiii, 99, 192, 195, 307, 506, 548, 1087, 1094, 1097, 1126 (twice). In the second edition, noun and adjective occur forty-nine times in twenty-five contexts; eleven of the contexts are part of prefatory, concluding, or bibliographic matter, and the remaining fourteen appear as part of patterning discussions like that on the relation between land and sea routes; *ibid.* (2d ed.), 1: 12, 21, 79, 84, 122, 259, 269, 270, 296, 325, 339, 360, 384, 387, 395, 407, 414, 416, 426, 454, 457, 463, 467, 545, and 2: 47, 62, 82, 95, 214, 223, 224, 514, 516, 517, 518, 519, 520, 544. The high frequency of the words' occurrence in introductory and summarizing statements allowed Braudel to be sharply polemic about structure and structuralism in general while remaining vague about the meaning of structure in given historical contexts.

rhetoric. But the drawback to this adroit tactic is its muddying of Braudel's general categories of historical explanation.

La Méditerranée has frequently been criticized for the lack of articulation among its three parts. The slow and fast temporalities to which Braudel directed attention and the different kinds of history (ecological, economic, and political) to which they chiefly relate seem to go separate ways: one does not influence the others. This problem, already present in the first edition, became more acute in the second, for the vagueness with which "structure" was used in practice was now reinforced by the necessity of avoiding an explanation of how "structure" functions with respect to new elements given causal force, such as "con-juncture." Braudel was obliged to remain vague and general, for any attempts to interrelate the kinds of history or the kinds of temporality in detail would have obliged an overall resolution of the differences between the two sources of his "structuralism"—the organic, sensorially oriented one of the 1930s and the more formalistic, abstract one of the 1960s.

Unwilling for these reasons to analyze how his two structuralisms might be associated rather than opposed, Braudel displaced the problem of conceptual coherence to one of representational coherence. On the latter terrain, that of his-torical narrative, he exhibited great skillfulness. As he had altered the concept of structure in the second edition, he made other adjustments to accommodate new tools of research and to aim at achieving an integrated text rather than at presenting an integrated argument about historical processes. Given that Braudel resolved the multiple tensions between his ideological commitments and his heuristic modes of procedure in this narrative manner, the relationship between Braudel's research strategy and his compositional strategy now needs to be considered.

HISTORICAL WRITING IS DUAL IN ITS AIMS. It must, to succeed, both *articulate* and *display* its subjects—both unfold a problem by presenting it serially yet inter-connectedly (organization) and seek the reader's involvement with this problem (rhetoric). With respect to organization, materials are arranged in Part 1 according to spatial systems; in Part 2, according to types of exchange.⁶⁰ Part 3

⁶⁰ The spatial systems (peninsulas, mountains, plains, seas, climates) are patterns whose structural rationale is based on unenunciated geological, meteorological, and biological principles—Braudelian geohistory, as discussed above. As in Part 1, the categories that pattern activity in Part 2 do not include any explicit structural argument as to why they, rather than other categories, have been chosen. In the first three chapters, economic elements of exchange have been set forth in their sixteenth-century patterns (monetary sources, price shifts, conditions of commercial communication, conditions of market control of production, demographic spacing and its shifts). The other four chapters deal with four noneconomic "rhythms" by extending the idea of exchange, so that all forms of human interaction can be understood as having their base in trading "goods"—political, moral, military, ideological, and esthetic, as well as economic. Braudel did not argue for this mode of understanding political, social, and cultural behavior. The list of subjects included in—and excluded from—his work implies it. He treated the subject of "empires" in chapter 4 by tracing the lines of Turkish and Spanish imperial expansion and counteraction, the subject of "civilizations" in chapter 5 by considering the permeability of cultural frontiers to technology and their impermeability to religious ideas and by outlining the geographical expansion of Baroque style from Roman and Spanish points of origin, the subject of "societies" in chapter 6 by delineating social mobility (primarily with respect to the "treason of the bourgeoisie" in its search for noble titles and in regard to the retention of socioeconomic supremacy by the nobility) and mar-

deals with groups of years, a decade or less in length, in chronological order. Braudel did not change this arrangement of *La Méditerranée* in the second edition. Increased emphasis on the “structural” approach to historical work did not alter the general disposition of subjects.

Braudel expressed his rhetorical strategy at the end of the second edition in a passage where he noted the difficulty of presenting the totality of temporal rhythms, fast and slow, that order human activity. The problem, as he saw it, is one of visual reproduction or representation. The “reality” of the past can be reproduced, he implied, insofar as the historian succeeds in re-presenting to the reader what has first presented itself to him in all of its plenitude: the temporalities’ “sum alone, apprehended by the group [*faisceau*] of human sciences—all of them retrospectively at the service of our craft—constitutes global history, whose *image* it is so difficult to reconstitute in its *plenitude*.”⁶¹ The global historian’s rhetoric cannot be merely or primarily that used for simple narrative, for unilinearly unfolding chronology: the global historian creates a sense of time’s plenitude by the use of graphic representations and imagistic prose.

Graphic representations of historical change are one of the chief attractions of the second edition of *La Méditerranée*. With the aid of Jacques Bertin and the cartographic laboratory of the École Pratique des Hautes Études, Braudel proved unusually adept at presenting immense bodies of documentation in summary visual form. The book’s pages become surfaces upon which he projected his understanding of temporal change as a series of spatial forms.⁶² Braudel reinforced spatial modes of thinking about the past (encouraged by the graphs) by rhetoric. As if he were gazing at charts, maps, or photographs, he wrote of the “asymmetry” of a political system, of the “center” of an economic movement, of the “general image” of population growth, of the conjunctural “grill” through which a historian can “see” social classes.⁶³

Braudel was, of course, far from advocating a naive positivism when he insisted upon the concreteness of verbal images. The elements of historical objects are constructs rather than physically existing things like pebbles or manuscripts. But the rhetorical means Braudel chose to present these elements encourages

ginality (the classes outside or along the borders of society: beggars, vagabonds, bandits), and the subject of “war” in chapter 7 by treating problems of defense perimeters and of the economic means to wage war (war financing, piracy, and privateering and its profits). Although never directly asserted, Braudel posited the fundamental quality of economic activity—and thus the dependency of other forms of activity upon it—not only by suggesting ways in which profit and loss, prosperity and poverty affected nearly every social problem but also by treating it first and at the greatest length. In the first edition, the three economic chapters that begin Part 2 are equal in total number of pages to the four chapters that treat all other aspects of “social history”; in the second edition, this disproportion is even greater: 260 pages for chapters 1–3, 200 pages for chapters 4–7.

⁶¹ Braudel, *La Méditerranée* (2d ed.), 2: 515 (italics added).

⁶² See, for example, *ibid.*, 2: figs. 56, 57, 58. Such illustrations help clarify the verbally vague use of conjuncture; see note 78 page 94, below. For other excellent examples of cartographic and geometrical inventiveness, see *ibid.* 1: 167, 193, 212, 336, 372. The illustrative material that Braudel had gathered for the first edition was eliminated from it at the last moment because of printing costs. But the dates and source references to the maps and graphs in the second edition indicate nearly total revision and substantial augmentation of this aspect of the work. For an excellent explanation and illustration of the techniques of the cartographic laboratory at the École Pratique des Hautes Études, whose services Braudel employed, see Jacques Bertin, *Sémiologie graphique: Les Diagrammes, le réseaux, les cartes* (Paris, 1967).

⁶³ See Braudel, *La Méditerranée* (2d ed.), 1: 451, 450, 368, 417.

readers to forget the difference between constructs and things: Braudel disposed his text as a series of externally seen phenomena "lighted up," as it were, by commentary that is by turns descriptive and contemplative. In a rather literal sense, Braudel's historiography is "reflective." The use of graphics, of aerial photographs, and of sixteenth-century maps and prints leads the reader to regard the past from the outside and from a distance.⁶⁴ Braudel thus broke sharply with the nineteenth-century tradition of presenting the past as a pattern of world-historical peoples whose invisibly imparted *spiritual* force, embodied by first one nation and then another and given form now by great political and again by great intellectual leaders, sweeps humanity forward to ever grander fulfillment. For Braudel, historical change is brought about by visibly ascertainable *material* forces impinging upon, colliding with, and limiting each other, such that each historical complex possesses a specific "weight"—that is, a specific ability to pass beyond the regular round of subsistence so as to increase the sources of energy, trade, and communication at the disposal of a given society.

Braudel's vision of the ultimate forces in historical life as a series of weights, light and heavy, superficial and profound, which set swirling the waters of time at different rates of speed, gives to material factors an active and even dramatic presence nearly unparalleled in previous historiography. The way in which physically existing things, natural and fabricated, orient the lives of men became the unifying theme of the first volume of Braudel's second major work, *Civilisation matérielle, économie, et capitalisme, XV^e-XVIII^e siècle*. Extending the lessons of *La Méditerranée*, Braudel has shown how the shape of a sail, the rate of pepper consumption, and the weight of woollens are both results and causes of productivity, communication, war, peace, and culture. Things in their simple material existence—or their lack—weigh upon the daily habits of masses of people in the same way that the location of ports, the amount of rainfall, and the system of roads determine and are determined by human interaction with the geographical milieu.⁶⁵

⁶⁴ See, for example, *ibid.*, 1: plates between 56 and 57, 232 and 233, and 312 and 313, and 2: plates opposite 176, 384.

⁶⁵ Volume 1 was published as *Civilisation matérielle et capitalisme (XV^e-XVIII^e siècle)* (Paris, 1967) and translated by Miriam Kochan as *Capitalism and Material Life, 1400-1800* (New York, 1973). In the preface to this work, Braudel spoke of writing a second volume dealing with "capitalism" and "economic life." When the work was completed two years ago, it consisted of three volumes under a somewhat more general title: *Civilisation matérielle, économie, et capitalisme, XV^e-XVIII^e siècle*. This mammoth work, running to some 1750 quarto pages, merits separate study; but several general comments are pertinent here. The subject of *Civilisation matérielle* is European economic activity between 1400 and 1800, the period of the world's "first modernity"; *ibid.*, 3: 543. The word "world" is used advisedly: Braudel compared European to many instances of non-European economic activity scattered over the globe in the three volumes. But, although this new work thus envisages a larger space and time than does *La Méditerranée*, its analytic scope is smaller because it is restricted to economic analysis. Moreover, Braudel has defined the "milieu" of economic action in social terms, not geo-ecological terms—that is, he has defined the global ensemble identifying the scope of his inquiry not as an interworking of social and natural, human and nonhuman forces but as the conglomerate of four systems, all of which he has called social in the largest sense. *Ibid.*, 2: 9. These four systems are (1) economy, (2) polity, (3) culture, and (4) society-as-hierarchy (classes, castes, and the like); *ibid.*, 2: 409. In practice, Braudel limited his choice of examples of the functioning of this global ensemble—or "ensemble of ensembles"—to a few privileged areas, above all to the urban areas of France, England, Italy, and the Netherlands. Moreover, aside from occasional remarks, he has treated the second, third, and fourth of these systems in their relation to the first only in one chapter; *ibid.*, 2: 407-518. Thus, *Civilisation matérielle* does not work out the patterns or structures of economic world history from 1400 to 1800; it offers rather a survey of the range of problems presented by economic ac-

To the presentation of the relative differences in material forces available at one moment of time or another, Braudel's second heuristic tool, the method of conjunctures, is admirably suited. Braudel emphasized this topographic and quantifying method in the second edition, well adapted as this method is to visually oriented understanding of temporal change. Indeed, Braudel's conjuncturalism, not his increased structuralism, most clearly separates the second edition from the first.⁶⁶

An economic conjuncture is a cycle of change, such as a rise and fall in the price of wheat.⁶⁷ Between 1850 and 1950 economists developed great sophistication in tracing these cycles, discovering among other things that seasonal (one-year) and short-term (five- to ten-year) fluctuations in prices were carried along or subtended by longer cycles of change.⁶⁸ Braudel generalized the idea of economic conjuncture by giving it the form of a quantifiable time-lapse on either side of a normative "center," so that conjuncture could be applied to any form of human activity by considering only the outline of the activity and its fluctuation away from or back toward its norm. The presuppositions for such a generalizing of the idea of economic conjuncture are two. All human activities are assumed to have this rhythmically undulating form rather than an erratic, or static, or steadily "upward" (progressive), or steadily "downward" (degenera-

tivity during these centuries. A representational, visualistic conception of historical reconstruction oriented the making of this survey as it did the composition of *La Méditerranée*: "C'est un essai pour voir d'ensemble tous ces spectacles . . . pour délimiter ce qu'est et a été la vie matérielle. . . . Sinon tout voir, au moins tout situer, et à l'échelle nécessaire du monde." *Ibid.*, 1: 494. Braudel also utilized the same chain of organic logic here as he did in *La Méditerranée*, which disposed him in most instances to give a "concrete," patterning definition to "structure" when he invoked that term. (But, for an example of structure used in the sense developed by structural social science, see *ibid.*, 3: 257.) He personified abstractions to make them more rhetorically concrete; indeed, he employed this technique even more frequently than he did in *La Méditerranée*. See, for example, the tissue of personifications articulating Braudel's statements about the behavior of secular trends in economic growth; *ibid.*, 3: 69. And note the following statement about capitalism, the chief "actor" in *Civilisation matérielle*; *ibid.*, 3: 540: "Capitalism has the capacity, at any moment, of swerving around: it is the secret of its vitality. . . . Changeable as it is, it is its own heir ad infinitum [*il se succède infiniment à lui-même*]." Braudel also retained his paradoxical cooption of social-scientific techniques and yet his rejection of social-scientific methods, affirming even more strongly than in *La Méditerranée* that the historian's concrete method is the principal path to truth in human affairs: "L'histoire [est] la seule [vérification] en vérité qui soit située hors de nos déductions abstraites"; *ibid.*, 3: 7. Also see, for a protest against how "philosophy, social science, and mathematics dehumanize history," *ibid.*, 1: 496. As in *La Méditerranée*, Braudel schematized this historical truth in a tripartite way. Thus, the volumes deal in order with material life, the market economy, and capitalism-as-counter-market; *ibid.*, 2: 197, 515. Or, to use the subtitles of the volumes, they deal in turn with "structures of daily life" (the most "profound" layer), "the play of exchange" (the complex, "alert," and changeable middle region), and "worldly time" (the deceptive and deceiving, but nevertheless commanding and "free," heights of international capitalist maneuvers). Analogies to the three times and the three parts of *La Méditerranée* are obvious enough, as are the shifts in emphasis toward more economically centered concerns. Of interest is that Braudel has given a somewhat more positive meaning to the "heights" than he did for the eventful layer of *La Méditerranée*. The pattern of Braudel's vision displayed in *Civilisation matérielle* is different, but the structural logic implicit in the pattern is the same as that in *La Méditerranée*.

⁶⁶ The text of *La Méditerranée*, however, combines this emphatically conjunctural practice with statements expressing reservations and lack of enthusiasm for the idea of conjuncture; see Braudel, *La Méditerranée* (2d ed.), 2: 520, 213.

⁶⁷ The development of "conjunctural" theory is primarily a French and German affair and does not always parallel the development of business-cycle theory in Anglo-American economic science. Braudel both expanded upon and contracted the economic theory of conjunctures in ways that require careful exegesis. My purpose is not to assess Braudel's contribution to economic conjuncturalism but to explore the consequences of his use of conjunctural method in *La Méditerranée*.

⁶⁸ For a convenient, although now dated, discussion of these and other conjunctural distinctions, see Joseph Schumpeter, *Business Cycles* (Cambridge, Mass., 1939), 165-74.

tive) form. And the categories to be measured, the subdivisions of human activity, are assumed to be unambiguously separable, so that cyclic rhythms in one category, such as culture, can be dissociated from those in another, such as economics. Braudel used the broad categorizations of human activity found in the secondary literature of his subject, and he took the rhythmically undulating, continuously unfolding notion of historical change for granted.

The names of conjunctures in *La Méditerranée* vary enormously; some are temporal, some spatial, and some simply nominal, taking the names of the different subjects of his narration. In terms of time, Braudel distinguished "very long," "long," and "short" conjunctures.⁶⁹ In terms of space, he distinguished between general and local conjunctures within the Mediterranean area and between northern European and Mediterranean conjunctures. He also referred to international conjunctures that united northern Europe and the Mediterranean and, still more generally, to "a 'conjunctural measure' of space in the sixteenth century," which delineates the rapidity with which people could travel.⁷⁰ The luxuriance with which Braudel developed the term "conjuncture" in the second edition is best exemplified by its employment in relation to different human activities.⁷¹ There are "mercantile," "business," and "large-scale commercial" conjunctures,⁷² demographic conjunctures, conjunctures of social mobility, conjunctures favoring large states, and general conjunctures of political "life."⁷³ There are "ideological" as opposed to "material" conjunctures, conjunctures of the "life" of civilizations, conjunctures of economic "life," and a "broadly conceived" economic conjuncture (*conjuncture au sens large*) governing political activity⁷⁴—as well as a dozen contexts in which the word is used either programmatically or too vaguely for its referents to be identified.⁷⁵ These profuse distinctions are partially regrouped into "very long" and "long" "economic" and "noneconomic" conjunctures in the concluding section of Part 2.⁷⁶ But the few passages in which conjunctures said to govern different types of human ac-

⁶⁹ The very long conjuncture to which Braudel alluded is two hundred years long, from 1450 to 1650, divided roughly in the middle (1550–60) into a "first," very prosperous, "rising" sixteenth century and a "second," less prosperous sixteenth century; *La Méditerranée* (2d ed.), 2: 216–17. The "long" conjunctures rise and fall within these two centuries in waves varying between thirty and forty-nine years; *ibid.*, 217–20. Short conjunctures are characteristically excluded from discussion with the comment that, although such "eventful research" as that identifying cycles of less than ten years has "great value," it has not been carried out completely enough as yet; *ibid.*, 220.

⁷⁰ Braudel, *La Méditerranée* (2d ed.), 1: 397, 426, 530, 331, and 2: 208, 215, 340.

⁷¹ Braudel used "conjuncture" or "conjunctural" 102 times in the second edition, but only 2 of these instances also appear in the first edition; compare *La Méditerranée* (1st ed.), 265, with *ibid.* (2d ed.), 1: 293, and *ibid.* (1st ed.), 508, with *ibid.* (2d ed.), 2: 10. I have not found any other instances of the noun or the adjective in the first edition.

⁷² Braudel, *La Méditerranée* (2d ed.), 1: 292, 404, and 2: 147. Among other economic types are gold-supply, silver-supply, and wheat-supply conjunctures, industrial conjunctures, conjunctures favoring peasant control of the land, and conjunctures favoring land control by real estate proprietors; see *ibid.*, 1: 431, 438, 454, 530, 538, 542, and 2: 219.

⁷³ Braudel, *La Méditerranée* (2d ed.), 2: 219, 45–50, 53, and 1: 296, 298, 305. There are also state budgetary conjunctures and conjunctures of war; *ibid.*, 2: 31, 215, 219. For an implicit conjuncture of war, although the word "conjuncture" does not appear, see *ibid.*, 164–66.

⁷⁴ Braudel, *La Méditerranée* (2d ed.), 2: 170–72, 95, 153, 219–20, 224, 28, and 1: 418.

⁷⁵ See, for example, *ibid.*, 1: 69, 274, 293, 322, 325, 369, 545, and 2: 224, 28.

⁷⁶ *Ibid.*, 2: 213–20.

tivity are associated with each other actually do little more than assert a connection without analyzing it.⁷⁷

Braudel's conjuncturalism thus creates a tangle of distinctions even as it aims to disentangle. The thrust of analysis is toward dismantling the unity of human action sufficiently to present its various aspects as a series of pictures, but the pictures in their multiplicity overlap each other in a kaleidoscopic jumble. Moreover, conjunctures remain merely contours of problems; they do not elucidate the problems' working parts. Because conjunctures in Braudel's work usually represent only the quantified variability of one kind of productivity or activity over time and not the covariance of several and because they do not encompass the mechanisms governing variability, his conjunctural analyses, particularly those concerning "long" and "very long" conjunctures, differ little in heuristic terms from his discussions of "structures." The addition of conjunctural to "structural" analysis introduces no new theoretical element into Braudel's conception of historical action; when coherently and generally carried out, conjunctural analysis can, at best, merely specify movement toward or away from change in a pattern.⁷⁸

The study of "events" does, however, add something heuristically new, for they are historical "realities" that have little or no pattern. "Events are dust," commented Braudel; they are infinite in number, they float here and there, scarcely touching the real soil of history.⁷⁹ The dust of history is particularly thick in the area of political affairs. But to what extent is this a consequence of Braudel's mode of approach to such affairs? He has treated diplomatic and military conflicts in accordance with geohistorical and exchange assumptions: war

⁷⁷ See, for example, the discussion of political conjunctures in relation to a "broadly conceived" economic conjuncture; *ibid.*, 2: 26–33.

⁷⁸ Thus, a negative conclusion about heuristic value should not obscure what must be a positive evaluation of the experimental value of Braudel's conjuncturalism. The idea of representing historical life as a series of time-flow charts of change in material quantities is an important one, even if the methodology remains vague and questionable for the means by which human activities are materialized and, hence, given not only analytically separate but even quantifiably calculable existence. With respect to the strictly economic use of conjunctural analysis, Braudel gave proof of greater care and specification outside the pages of *La Méditerranée*. In collaboration with the English scholar Frank Spooner, Braudel published a wide-ranging but precise chapter on conjunctures for the *Cambridge Economic History*; see Braudel and Spooner, "Prices in Europe from 1450 to 1750," in E. E. Rich and C. M. Wilson, eds., *Cambridge Economic History*, 4 (Cambridge, 1967): 374–485. Braudel's interest in conjunctures has not flagged. In his most recent work, he has spoken of "conjunctural history" as the "only" method capable of "lighting up" the path to knowledge of the temporal development of those "historical monsters," the "world economies"; *Civilisation matérielle*, 3: 56. Here, as in *La Méditerranée*, he has written about many different kinds of conjunctures (cultural, political, social, and so on) in a variety of temporal lengths. His predilection for the longer cycles has, however, remained: the longer "fluctuations and slow oscillations . . . are the 'indicators' that we need"; *ibid.* They are indeed "a rule of world history," something that came into being long ago and that is "destined to perpetuity"; *ibid.*, 3: 528. But, again as in *La Méditerranée*, he has spent very little time explaining the mutual relations among diverse conjunctures. It seems to have been difficult for him here, as elsewhere, to perceive the interest in or need of making more precise, instead of more grandiose and obscure, the historical problems and concerns that he has addressed. There are always more and longer conjunctures to be considered. Braudel's conjuncturalism is thus ideologically related to his commitment to "grand" history and to his aversion to "petty" facts.

⁷⁹ Braudel's striking formula occurs at the beginning of Part 3; see *La Méditerranée* (2d ed.), 2: 223. The contempt implicit in this phrasing is somewhat modified in subsequent paragraphs. But the image is sufficiently repeated and supported in other asides to indicate that this phrase, initiating discourse on the details of political and military change in the later sixteenth century, was not an exception, either in formulation or in belief. For some of these asides, ("poussière d'événements," "poussière d'actes," "poussière de faits divers"), see *ibid.*, 77, 96, and *ibid.* (1st ed.), 721.

and the causes of war become problems of logistics—problems of defense perimeters, food supply, arms production, soldierly demography, and access to information. These ways of dealing with political history do tend to reduce it to “dust,” but they are not, after all, the only possible ways of studying diplomacy and war. Had Braudel pursued questions of political ideology or the development of administrative institutions, would he have discovered systems of long or very long duration in these dispersed and fragmented regions of inquiry?⁸⁰ It is as if Braudel felt ideologically constrained to prove—by denuding political, institutional, and military history of long-term problems and neglecting the impressive historiographical models in these areas—just how petty eventful history is.⁸¹

Even more serious is Braudel’s neglect of events that break patterns, a neglect encouraged by treating events as if they were atoms, each independent of the next, in an evenly unfolding chroniclelike sequence. The sixteenth century did not lack events that broke patterns; insurrections, for example, were not rare. But such events were historically negligible for Braudel, because they had few consequences. In Braudelian terms, they failed. Yet the sixteenth century had a successful revolution that affected the entire Mediterranean area, and that rebellion was not unrelated, many would say, to the myriad of insurrections, urban and rural, that had seemingly failed. This revolution, somewhat awkwardly and reductively named the “Reformation,” broke the hitherto integrated institutional hold of Roman Catholicism over Western European Christians. Braudel examined the repercussions of the series of events involved in this revolution only in their outer contours and thus reduced the effects of the religious reform movements—Protestant, Catholic, spiritualist, mystic, and so on—to the ricochet of diplomatic policies and the rattle of rival ideological styles in propaganda and art. Events as breaks, as differentiations instead of repetitions, are as difficult to accommodate within the terms of Braudel’s method and the emphases of his style as are the consequences of social deviation from anonymously human norms.⁸² Can patterns (“structures”) be distinguished without also examin-

⁸⁰ Just as there is a conceptual bias involved in presenting politics as the petty, there is another involved in presenting geo-ecological factors as consistently grand. Short-term conjunctures are absent from the geohistorical sections of *La Méditerranée*. Jean-Claude Raison’s conclusion is, therefore, just: “L’étude de l’évolution des systèmes spatiaux dans le temps [est] moins clairement mise en valeur par Fernand Braudel”; Raison, “Géographie humaine,” in R. Chartier *et al.*, eds., *La Nouvelle histoire* (Paris, 1978), 184 (italics added). The ecology and geography with which humans interact are subject to “events” as well, as the poisoning of our air and water attests; many aspects of this profound level of historical life are no less “alert” to changes than the quickly moving sectors of political, economic, and cultural history.

⁸¹ Braudel handled some patterning as opposed to eventful aspects of politics and war in Part 2, chapter 4 (“Empires”) and chapter 7 (“The Forms of War”). But he had the models to do more. In the area of long-term political and military institutional developments, works by Roger Doucet, Roland Mousnier, Juan-Luis Vives, J. R. Hale, Garrett Mattingly, and Frederico Chabod are among the many excellent historiographical examples for early modern Europe, all of which were available by the time Braudel revised *La Méditerranée*; and by the early 1960s Friedrich Meinecke, Rudolf von Albertini, P. Mesnard, J. W. Allen, J. G. A. Pocock, and others had delineated well the long-term development of political ideology in the times and places that Braudel treated.

⁸² Braudel’s approach is wonderful for evoking deviance (nomadism, banditry, vagrancy) but not for pursuing the perspectives he had opened up. Examples of deviance serve the function of centering and unifying his discourse rather than of upsetting and calling into question his themes. Thus, far from calling the rule into question, the exceptional and deviant either mark the limit of extension of a historical regularity (the eco-

ing the events that made or broke them? Can the reversal of a conjunctural curve be explained without also exploring the particular situation and the accumulation of circumstances that led to interruption of the cycle's course?

The problem lies in the concept of event itself. Braudel has tended to assimilate "event" to "fact," even to "occasional fact" (*fait divers*). Events are "dust" because facts are legion; the facts swarm out of the historian's primary material in such numbers that their affinities to each other seem countless. But events are not facts. They are not "given" as such, either in the archives or in human experience. Events are the historian's construction, like conjunctures, patterns, structures, and configurations. They do not constitute the point of departure in historical research, a "surface" that must be penetrated to seize the past's true being. Far from representing ultimate atoms of behavior that obscure the shape of history's totality, as trees do the forest, events are intersections, moments of clash that reveal the disharmonies in configurational patterns. An event is a site of change, a construction developed by historians because they seek to understand structures and conjunctures, not despite their larger goals.⁸³

The crossroads of historical inquiry, the first point of construction of temporal-spatial ensembles, is not the discovery of facts or the isolation of important as opposed to unimportant events⁸⁴ but the assemblage of "situations." In the context of structural and conjunctural research, a situation is a sketch based upon a first gathering and classification of materials. Outlining the situation to be investigated enables the historian to form hypotheses about patterns and conjunctures, which are tested by moving in two directions, toward patterning

nomic power of a city, the political power of an empire) or suggest a world beyond the limits of the subject at hand, a world that Braudel has left to others to explore and relate to what he has done.

⁸³ Braudel seems to have been hinting at such a concept of event in the concluding paragraph of Part 2 in the second edition. With reference to short-term conjunctural crises, he claimed, "L'étude de ces crises courtes, de ces houles violentes, de leur extension et surtout de leur nature variable jalonne l'évolution de l'économie méditerranéenne. Cette recherche événementielle, poussée en profondeur, aurait sa grande valeur. Mais elle reste à faire." *La Méditerranée* (2d ed.), 2: 220. The "realism" of Braudel's historical method is what chiefly prevented him from developing an analysis of events in relation to structures, conjunctures, and patterns. The pressure of the ideological imperative of "reality" and "concreteness" has led him to accentuate, for example, a difference between "event" and "occasional fact" in his more recent work, something he did not do in *La Méditerranée*. He explained the difference, à propos of his new work, in an interview just over a year ago: "L'événement, c'est un fait divers qui fait du bruit. Je préfère quant à moi le simple fait divers qui n'en fait pas, parce qu'il se reproduit." The occasional fact is "l'indicateur d'une réalité longue, et quelquefois, merveilleusement, d'une structure." The event has less reality and less indicative power because it has been contaminated, "grossi par les impressions de témoins, par les illusions des historiens. On grossira l'entrée d'Henri IV à Paris." *Le Monde*, December 14, 1979, p. 23. Against this disgust with illusory events and this subordination of occasional facts to their usefulness as indicators of the long, unbroken continuities of history, placing Marc Bloch's sense of the complementarity of the historian's concern with discontinuities as well as continuities, with individual moments as well as general evolution, is appropriate: "The historian never escapes from time. But, in an inevitable oscillation . . . , he sometimes considers the great waves of related phenomena which run over long periods, and sometimes the specific moments in which these currents are channeled into the powerful vortex of direct experience." Bloch, *The Historian's Craft*, trans. P. Putnam (New York, 1953), 156.

⁸⁴ Braudel affirmed that the way to use research into events in order to write history of the kind recounted in Part 3 of *La Méditerranée* is to divide the important from the unimportant: "Tout d'abord, l'histoire ainsi conçue ne retient que les événements 'importants' et ne bâtit que sur ces points solides, ou présentés comme tels. Cette importance est évidemment matière à discussion." After mentioning several criteria used by other historians (Voltaire, Michelet, Taine), Braudel offered the criterion of importance that he used: "Enfin est important tout événement qui est lié à des antécédents et qui a des suites, tout événement qui est pris dans une chaîne. Mais cette histoire 'sérielle' est le fruit elle-même d'un choix, fait par l'historien, ou pour lui par les sources documentaires essentielles." *La Méditerranée* (2d ed.), 2: 223-24.

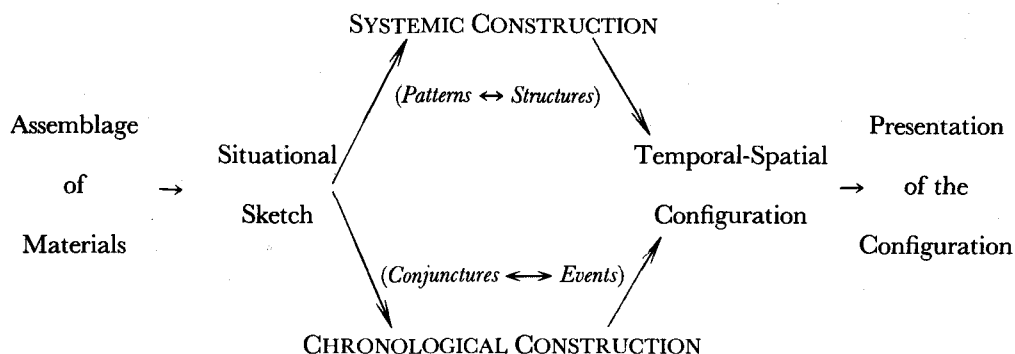
and structural analysis and toward conjunctural and eventful analysis. The first direction can be called "systemic" and the second "chronological." Neither can be carried very far without reference to the other. Historians can discover when a pattern changes by reference to the concerted flow of conjunctural curves, long and short, which measure shifts in the relation of one kind of human activity to others.⁸⁵ And they can discover why and how a pattern changes by analyzing the systems in play (the other patterns associated with the pattern in question and the structures implied in them) at the moment when conjunctural curves change their course—that is, at the moment when "productivity" of one form of human activity or another, insofar as such activity is quantifiable,⁸⁶ grows or declines massively. Finally, analyzing particular times and places allows historians to construct the "events"—the breaks in routines, customs, ideas, technologies—that disrupt patterns, sometimes massively enough to indicate that a structure has been displaced from its dominant position or has disappeared entirely.

Just as there can be no adequate patterning without structural analysis, there can be no adequate conjunctural analysis without theorizing about "break-points" or "events." How many break-points historians construct depends not upon the wealth or dearth of documents in archives but upon the historian's chosen focus of inquiry. The analytic procedures of the structural historian are complex, but they are not necessarily mammoth in quantity. And the results achieved can never be called global or total because they depend upon an intertwining of techniques, each of which could be further refined and subdivided. Much like the process by which structures and patterns become systems of systems, conjunctures become rhythms of rhythms: longer and shorter curves can always be constructed if documents are available, and more and more curves can be placed in covariance. "Events" themselves are also capable of refinements in scope and detail: any given break can lead the historian toward discovery, analysis, historical reconstruction of smaller ones; any revolution can turn out to be preceded and followed by innumerable insurrections and reforms.

Stopping-points in historians' work, therefore, can only be incidental, not definitive. They begin their research at a certain moment in the history of the investigation of their chosen field and they end at another, so that they cannot benefit from later studies. They move (see diagram) from research (assemblage and sketch) to an analytic stage, which should include construction of multiple systemic and chronological models (patterns and structures; conjunctures and

⁸⁵ As indicated above, one conjunctural curve isolated from others tells little. The main point of conjunctural analysis is to indicate conjoint movement, the covariation of several or even many kinds of production or action: the prices of commodities in relation to changes in demography; the increase in book production in relation to literacy.

⁸⁶ All human activities might be said to have a quantifiable aspect, although this aspect may be relatively trivial (how many people perform the activity) or inaccessible (imperfect records or, more seriously, the imperfection of the tools needed to separate analytically one aspect of human behavior from another). Thus, "seizing the past" in its quantifiable aspect may not lead to understanding its most important side and certainly not its only historically verifiable side. Quantification and conjuncturalization of past human activity must be tried everywhere, but the successes as well as the failures of such inquiry should be studied for their epistemological implications, not merely accepted with delight or despair, depending on the magnitude and precision of the available numbers.



events), and finally to historiographical synthesis, the presentation to the reader of the configuration of a certain place and time, with its relatively harmonious or disjunctive tendencies of evolution.⁸⁷

STRUCTURE, CONJUNCTURE, EVENT. Because Braudel accommodated the economic concepts of structure and conjuncture to the organic framework of the first edition of *La Méditerranée*, he in effect changed their meaning from analytic and specifically economic to descriptive and generalizing terms. Thus altered, these concepts fit together harmoniously with eventfulness or short-lived temporality, as the ill-assorted old trinity that the three terms displaced—geographic time, social time, and individual time—had not.⁸⁸ The apparently unimportant place of eventful history in this schema—a mere point of departure, a launching pad toward deeper realities—proves to be crucial. For these despised, dusty events offer points of stability for the historian in the search for a “concrete” and “total” past; the rapidity with which event follows event allows the historian to perceive the relatively slower tempos of conjunctures and “structures.”

Braudel claimed that he measured everything by the slow rhythm of struc-

⁸⁷ The diagram disregards, for reasons of visual clarity, the reflexive returns that take place at all stages of the process, compelling the historian to renew his research, revise his writing, and alter his analyses. This reflexivity is, however, represented in the cases of patterns vis-à-vis structures and of conjunctures vis-à-vis events by the double-pointed arrows connecting these items.

⁸⁸ Braudel began harmonizing “structure,” “conjuncture,” and “event” by reducing their social and augmenting their temporal content in two important essays published in 1958, “Histoire et sciences sociales: La Longue durée” and “Histoire et sociologie.” In the latter article, he provided this formula for the relationship among these three terms: “History is situated at different levels. . . . At the surface and eventful history is written in short temporality. . . . Halfway down, conjunctural history follows a broader, slower rhythm. . . . Beyond this “recitative” of conjunctures, structural history or history of the long term summons whole centuries; it is on the boundary between the mobile and immobile and . . . appears invariant in relation to other histories that more quickly flow away . . . and that in sum gravitate around it.” “Histoire et sociologie,” 112. The concluding passage of the second edition of *La Méditerranée* indicates that Braudel had not changed his way of expressing the differences among the major divisions of historical analysis when he revised his work in the 1960s. For the concluding passage, see note 43 page 80, above.

tural history.⁸⁹ But what is the measuring stick that determines that structural history is slow? Braudel's sum of temporalities presupposes an evenly measurable "beat" of time: "Long-term duration, conjuncture, event easily enclose each other [*s'emboîtent sans difficulté*], because they are all measured on the same scale." That scale, Braudel wrote, is the "irreversible" scale of "historical time," a "world-time" universal and absolute, "which moves with the very rhythm of the rotations of the earth."⁹⁰ Braudel's concept of the temporalities making up history is metaphysically anchored, attributable to the very nature of the world. And his three metasigns—Time, Space, Man—stand like sentinels at the edges of Braudel's historical vision; they are the absolutes guaranteeing the rest. As the organic-humanistic concept of Man shaped Braudel's ideas of space (that restless giant, the Mediterranean) and time (the "structural" soil in which Man works, the eventful air that Man respires), so Braudel's visual-sensory concept of Space influenced his notions of humanity and temporality. "Short" and "fast" time are not geometrically abstract notions to Braudel, but visual, actual, experiential concepts, physically "there" like the earth's rotation. Braudel's "world-time" is not that of philosophers or scientists who seek some Archimedean point beyond the variants of man-made clocks and earth-based calendars. The timing that Braudel used is a humanly anchored vision, the product of a historian's reflections about the ideal tempo for human affairs.

Structural time, the time that lies at the "bottom" of things, is a time created by a spatial tactic—that is, by looking at affairs from a distance, so that particulars recede and only the repetitious, "anonymous" contour of men's daily actions can be perceived. From this perspective, time for most people, most of the time, passes too slowly, in a wearisome round that encloses them "in the infinite perspective of long-term temporalities."⁹¹ Upon closer inspection, one must conclude that Braudel's spatial concept of fast and slow times, times that entrap men either in the eventful surface or in the structural depths of historical process, reflects the experiences of a certain man living in a certain configuration, the highly disjunctive configuration of a Europe convulsed by the monumental events of the Great Depression and World War II and by conjunctures like totalitarianism and the "decline of the West."⁹² The concept of these temporalities was also marked by scientific conjunctures, most obviously by the relative efficacy or uselessness of the tools available to a historian who in the 1930s and 1940s envisioned history as a synthesis of any and all social-scientific approaches. It is, therefore, difficult to endow with metaphysical absoluteness the world-time implied by the fretful ubiquity of too fast and too slow rhythms, that

⁸⁹ "Even for the conjunctural history of crises, one must often say, structure, slow history, comes first. Everything must be measured by this essential sea-level [*ce plan d'eau essentiel*]." Braudel, *La Méditerranée* (2d ed.), 2: 518.

⁹⁰ Braudel, "Histoire et sociologie," 117. Braudel evoked the *emboîtement* again on the last page of *La Méditerranée* in a phrase complementing the implied idea that "structures" are very long conjunctures. Here he defined conjuncture as a "groupement d'événements de même signe"; *La Méditerranée* (2d ed.), 2: 520.

⁹¹ Braudel, *La Méditerranée* (2d ed.), 2: 520. To find "slow" time, one must look from the "side" or gaze into the "depths." See, for example, note 93 page 100, below.

⁹² With the latter, Spenglerian phrase, I refer to the psychological impact upon several European generations of the destructiveness of the world wars and of the emergence of non-European political powers.

never-never time in which human affairs will deliciously, deliberately unfold at a perfect pace.⁹³

Time, then, possesses a less absolute status in Braudel's historical reconstruction than his other two metasigns; its description, its forms, its behavior derive from the organic-humanistic and spatial-visual codes otherwise organizing his work. Time's sum is a space: "global" history fills the bowl of human possibility, and to explain that reality Braudel resorted, ever and again, to his most precious image, the sea, the Mediterranean, "passionately loved."⁹⁴ In the second edition, where the foundation of historical process in "structure" became more difficult to exhibit concretely, the grandeur of the sea became a verbal substitute for direct observation: the *subject* of the work, the history of the Mediterranean, became its *object*, a metaphorical space in which all elements of historical life could be held together and contemplated. Inside the rigid rim of historicity represented by structurality, the interior of the totality of history could become a liquid whose function is, like that of the sea, to beckon people from adjacent shores to move and meet each other across neutral depths of shifting water. In Braudel's time-world, all change is exchange: merchants, conquerors, explorers came to trade guns, goods, and ideas, seeking to learn, to profit, and to conquer. If time is space and space is sealike, agitated solubility, then particular men and particular social groups are the flotsam and jetsam of its tides, now meeting and joining, now clashing and turning away.

The insignificance of particular persons and groups, which this arrangement of the past entails, is softened by the thought of the grand totalities to which such details contribute. Men make Man, the temporalities form History. An organism grows ever larger, marking its presence with ever grander gestures. When "grand history" came to an end in the Mediterranean area, so does Braudel's examination of it; the last chapter is entitled "The Exit of the Mediterranean from Grand History." Braudel's contourlike method of establishing the presence of structures, conjunctures, and events is an enlarging totality, moving constantly away from, not toward, the intricacies of the particular.⁹⁵

⁹³ The point implied here can, of course, be carried further. There are no intrinsically "long" and "short" times: measuring units of time, like space, vary with cultural tradition, group experience, and individual experience because they are socially, historically, and psychologically—as well as physically—determined. Braudel himself has emphasized on occasion sociohistorical factors in explaining the impact of World War II upon his work: "Events" in the prison camp where he was forced to experience the war passed too quickly, he has written, and so he "struggled . . . to escape the chronicle of those difficult years" (1940–45). If wartime events, as experienced in the camp, became one of the paradigms for Braudel's image of fast time, they also motivated construction of the opposite image, a slow, "structural" time, for, as he further explained, "to refuse events and the temporality that events represent [*refuser les événements et le temps des événements*] allowed one to put oneself to the side, in shelter, so as to gaze at them from a distance . . . and not believe in them too much." "Histoire et sociologie," 116.

⁹⁴ The first sentence of Braudel's preface to the first edition begins, "J'ai passionnément aimé la Méditerranée"; *La Méditerranée* (1st ed.), vii.

⁹⁵ Braudel, *La Méditerranée* (2d ed.), 2: 469. For this point in general form at the end of the work, see *ibid.*, 2: 512: "À sa façon, la décadence de la grande guerre est comme le signe avant-coureur de la décadence même de la Méditerranée." In the first edition, Braudel carried this point forward in a conclusion eliminated from the second edition; see *ibid.* (1st ed.), 1084: "Il me semble par exemple que l'Atlantique est au centre du monde actuel—mais pour combien de temps encore—un peu comme la Mer Intérieure était jadis au cœur du Vieux Monde." This role of grandeur played by the sea in *La Méditerranée* is given to capitalism in *Civilisation matérielle*; in the later work grand history and the history of capitalism are nearly synonymous, it would seem: "J'ai

The effect of moving outward rather than inward in search of the meaning of historical action is to play out the cords that weave connections between words and the things to which they refer. It loosens these connections, and the consequences are twofold. On the one hand, it increases the flexibility with which meaning can be established. A phenomenon may be given single or plural significance depending on the design of the text. On the other, it makes the meaning of any particular historical phenomenon, and even of any particular group of historical phenomena, ambivalent and imprecise—so imprecise, finally, that with sufficient enlargement of one's perspectives only verbal concreteness remains. The word-webbing is concrete, but the relation of words to their referents becomes slippery and vague, the more verbal coherence is sought. Increased attention to verbal coherence was perhaps implicit in the decision to make a second edition. Braudel's methodological tools had sharpened by 1966, particularly in the area of investigation that he called "conjunctures." But this improvement led to an impasse; there was no "bottom" to it, no "length" to which he could not have gone. Geography no longer provided the stopping point; "structure" took its place. But "structure" is a categorizing word, not a referential noun like sea, and forms part of a system of argument, not one among many illustrations and examples of the system.

Braudel, however, refused to accept the status of "structure" as an analytic rather than a referential term, yet he also accepted it, because he was constrained by his choice of research strategies both to accept and negate modern social-scientific structuralism. What is true of "structure" is true of all of Braudel's categorizing terms: they form part of a system of argument and are nevertheless presented as facts, as "realities" for which no argument is needed. In the end, Braudel's "concreteness" is metaphysical; the order that he attributed to history is that of a "global order" of definitions: "To say that the economic and the political can be classified in short-term or even *very* short-term time better than can other social realities is already a way of sketching a global order that surpasses them." The economic, the political, and other social realities—terms like civilization, science, religion, and war—are "sketched" in Braudel's text, traced in outline, given long- or short-term form, but never articulated in relation to each other, never argued for systematically. To Braudel these "categories" are already articulated and ordered; the historian's duty is merely to find them, "to search, beyond what they [economic and political affairs] contain of eventfulness, for structures, for categories."⁹⁶

soutenu, au cours de cet ouvrage, qu'un capitalisme *en puissance* s'esquisse dès l'aube de la grande histoire, se développe et se perpétue des siècles durant. C'est Theodor Mommsen qui a raison. C'est Michael Rostowtzeff qui a raison. C'est Henri Pirenne qui a raison. ... Parfois aussi, rarement, de grandes ruptures interviennent. La Révolution industrielle en est une, assurément. Mais je soutiens, à tort ou à raison, qu'au travers de cette grande mutation, le capitalisme est resté, pour l'essentiel, semblable à lui-même." *Civilisation matérielle*, 3: 538. On the capacity of capitalism to reproduce itself, also see note 65 pages 91–92, above.

⁹⁶ For the quotations in this paragraph, taken from the introduction to Part 3, see Braudel, *La Méditerranée* (2d ed.), 2: 224. The verbal nets used to hold together the far-flung topics of *Civilisation matérielle* are similar. They are based on the interplay of the three tenets emphasized in the text: history-as-life, concrete and real, is that of men meeting, circulating, and exchanging goods in economic centers; history-as-organic-life insures the continuity of such activity and indeed presupposes the presence of abiding entities called "structures" that, in effect, constitute the rules of exchange; and history-as-organic-life-that-grows allows for the "structures," and

The ambivalent grammar of this last phrase (Are “categories” the same as or other than “structures”?) is seemingly clarified by Braudel’s suggestion that he might have inverted his narrative order by beginning with events, “arriving afterwards” at structures, “and then at permanences.” Events-structures-categories, fast time-slow time-permanence: categories are “permanences” that do not move, at least as long as they are not subjected to the same historical methods that are applied to less grand, timelessly conceived entities. Given his *point d’appui* of categories that need not be critically examined because they comprise permanently true Ideas, Braudel quite clearly felt little inclination to explore, for example, the ambivalence that his structuralism entailed, the conceptual tangles that his conjunctures contained, or the reductions that his events-facts comprised. Founding the text on the permanence of categories, although that allowed its surface to exhibit admirably complete coherence, precluded any effort to deal with, perhaps precluded any apprehension of, the permanent questionability of terminology.⁹⁷

If *La Méditerranée* has nevertheless been called a masterpiece by so many, it is not least of all because Braudel’s categories are used so unquestioningly, so approvingly, by nearly all of us and especially by the French. For, if many commonplaces of modern social science—family, state, social class, capitalism—have frozen over the areas they purport to describe to such an extent that an Occidental eye glides over them imperturbably, there are others whose coded connections, peculiar to this or that national tradition, cause a stranger to stumble and turn again to stare at the offending semantemes.

These stumbling blocks for the foreigner, picking his way through French sociohistorical science, lie half-buried in the organic tradition. The dominance of organic thinking in French “human science” is echoed in Braudel’s work, just

all that they carry upon their profound waves, to grow and change, becoming ever more grandiose. Thus, the continuity of capitalist history is taken up in a continuity larger-than-which-none-can-be-thought, the history of “growth” or constant expansion. Just as the history of capitalism is a far vaster subject than that of industrialization, so the history of the world’s “first modernity”—that is, the rise of capitalism—is less vast than that of economic growth generally, since the latter is coterminous with historicity: “*La modernisation ... est ... un ensemble plus vaste que l’industrialization. ... Et le champ de la croissance est plus vaste encore: elle emporte, avec elle, la globalité de l’histoire.*” *Civilisation matérielle*, 3: 510.

⁹⁷ For this point made in similar terms, with particular reference to Braudel’s three enfolded temporalities, see Gérard Mairé, *Le discours et l’historique* (Paris, 1974), 164. In today’s professional history, Mairé has written, the “logical form” of the historian’s discourse typically is not submitted to analysis and, because of that, “le discours ... impose littéralement aux contenus qu’il expose un caractère spéculatif.” Hence, the historian’s way of knowing the past (the systemic logic, or structure, of his research method) becomes the structure of history itself: “La structure de savoir est la structure de l’histoire. Le mot devient la chose.” Thus, Mairé concluded, “Dans l’exemple ‘classique’ de *La Méditerranée* ... les plans de l’histoire [that is, structure, conjuncture, event] sont arbitraires, ce sont les plans de l’historien. Ils ne sont pas spécifiques, de sorte que la spécificité de leur durée n’est pas prouvée, et leur différence ne présente aucune nécessité historique. C’est une manière d’écrire l’histoire.” *Ibid.*, 210. In *Civilisation matérielle*, Braudel has undertaken a discussion of some of his terminological distinctions. He is generally less hostile to theorization and more attentive to the manner in which words direct inquiry in this new work, although appeals to “reality” and exaltation of the “concrete” remain prominent. For example, the third chapter of Volume 3 divides European “time” and “space” as a function of “world-economy” and “conjuncture,” and Braudel discussed the way in which various scholars have constructed the “suitable vocabulary” that they provide. *Civilisation matérielle*, 3: 11. But the general goal of even such discussion remains less that of analyzing historical discourse than of ordering it, so that each phenomenon can be put in its place: “Sinon tout voir, au moins tout situer. ... Essayer de classer, de mettre de l’ordre, de ramener une matière disparate aux grandes lignes, aux simplifications de l’explication historique.” *Ibid.*, 1: 494.

as more generally the *Annales* school has developed its avant-garde economic, mercantile, and material concerns in ways that conciliate rather than clash with the urbane organic humanism of French tradition. Behind Braudel stand Bloch and Febvre; behind the first *Annalistes*, Pirenne and Vidal; and behind the organic science of the early twentieth century, the resounding truths of Romantic historiography with its glorification of the nation as the heart, the voice, the soul of a people. Finally, behind Michelet, Thierry, and Guizot lies the human science of Condorcet, Rousseau, Diderot, and other patriarchs of the Enlightenment. Here in the eighteenth century are the mediate sources, the most recent and relevant transform of a humanistic-organic structure of Western thought as old as or older than Platonism.⁹⁸ This pattern or, as Braudel might have said, this long-enduring "structure" of French intellectual life accounts for the main ideological features of *La Méditerranée* and for the immediacy with which Braudel's work was recognized in 1947 as "marking an epoch."⁹⁹

THE GEOHISTORICAL ORIENTATION of *La Méditerranée* dislodged the state from its role as the focal point of historical inquiry, making politics secondary to other historical ensembles of action in ways that economic and cultural historians had in vain tried to accomplish during the preceding hundred years. *La Méditerranée* focused on a space ecologically articulated rather than on a nation politically expressed, relating mercantile and agricultural activity to spatial systems rather than to state laws, breaking with the nationalistic overemphasis characteristic of the older French historiographical tradition, and undermining the tendency of Western historiography since antiquity to understand the past as the thoughts and actions of state-building elites. These displacements go far toward justifying the encomia of Febvre, Labrousse, and others in greeting *La Méditerranée* as an epochal work. But each victory can entail losses, each step forward often requires the abandonment or omission of other paths. In Braudel's hands, the diminution of the political aggrandized the economic, regard for the large and encompassing as uniquely grand and important misrepresented the small as

⁹⁸ The term "human science" seems to date from the 1770s and "social science" from the 1780s in France. On this point and on the relationship between the social and the historical sciences in the Enlightenment, see Keith Baker, *Condorcet* (Chicago, 1975), esp. 197–204; and Georges Gusdorf, *Les Principes de la pensée au siècle des lumières* (Paris, 1971), esp. 193–212. I am not, of course, suggesting that Enlightenment social science was uniquely organic but only that the organic mode was one ideological strain among many in the eighteenth century and that this strain became predominant in the nineteenth century in the sociohistorical tradition to which *Annales* historiography belongs. The trinity of time, space, and man that Braudel used in association with organic-humanistic rhetoric invokes a thought-pattern somewhat older historically than the Enlightenment. The assumptions of a homogeneous space, a homogeneous time, and a homogeneous human nature, harmoniously fitted to each other, became "self-evident" between the sixteenth and eighteenth centuries in Europe, displacing the older religious dualism of nature and supernature. Robert Nisbet has illuminated some of the phases and features of the humanistic-organic structure of Western thought but has treated the question more narrowly than I have done by tracing only the use of what he has called the "metaphor of growth." Nisbet's analysis remains posed in the terms of the very structure he has studied, as, for example, in the eminently organic distinction that he has maintained between "external events" and "intrusions," a distinction he has proposed as an alternative to the "metaphor of growth." See Nisbet, *Social Change and History* (New York, 1969), esp. 280.

⁹⁹ For this comment of the Sorbonne jury, see note 5 page 64, above.

petty. Far more than a critique of one kind of historical tradition in the name of another, the tendency to equate politics with events, while it makes possible a "view" of "all" of the Mediterranean, curtails that vision, encouraging vagueness frequently to pose as grandeur.

La Méditerranée is not in the narrow sense a paradigm or an exemplary work. Nor has it been literally imitated by anyone, because it offers an image of *Annales* history more than a method. It played a role at a certain moment in centering scholarly energies ideologically, offering a direction for research and a place to carry it out—the Sixth Section of the École Pratique des Hautes Études—that were elsewhere lacking. *La Méditerranée* constructed a new and grander vision of two traditional fields of historical work, geohistory and economic history, and within this grand space there was ample room for the development of individual projects.

These two orientations of research have, in fact, been developed by *Annales* in different directions during the years since *La Méditerranée* appeared. Geo-ecological analysis has widened its temporal scope while narrowing its subject-matter with Emmanuel Le Roy Ladurie's *Histoire du climat depuis l'an mille* (1967). Economic research tended to dissociate itself from structural considerations and to devote itself to conjunctural and serial methods, as indicated, for example, in Pierre and Huguette Chaunu's eight-volume *Séville et l'Atlantique, 1504–1650* (1955–59). In contrast to these limited developments of Braudel's initiatives, research into "material civilization"—that is, into the materially mediated, "weighty," or repetitious customs of people—has erupted in a galaxy of fruitful directions, as revealed, for example, in the work of Jacques LeGoff and Le Roy Ladurie on folk culture in medieval towns and villages, of André Burguière and Jean-Louis Flandrin on contraception, marriage, and demographic change, and of Jean-Jacques Hémardinquer on dietary customs.¹⁰⁰ Insofar as these historiographic innovations developed in the wake of Braudel's work, *La Méditerranée* may be seen as a patterning element acting on the work of others in such a way as to extend the author's historicization of new, nonpolitically centered ensembles of social action. The rhetoric of space with its intoxicating vastness, of exchange with its ceaseless activity, and of life with its alluring warmth have inspired many others to construct equally new and compelling visions of the past.

Fernand Braudel's work, then, gave to the group of historians loosely called the *Annales* school something more precious than a model. He offered to them, and to all of his readers, what may be called an inspiring suggestiveness. Braudel's prose persuades, entices, commands the reader's assent with its easy roll, its repetitious lilt, its polysemic richness. The coruscating movement from subject

¹⁰⁰ LeGoff, *Pour un autre Moyen Âge* (Paris, 1978); Le Roy Ladurie, *Montaillou, village occitan de 1294 à 1324* (Paris, 1975); Burguière, "De Malthus à Max Weber: Le Mariage et l'esprit d'entreprise," *Annales*, 27 (1972): 1128–38, and *Bretons de Plozévet* (Paris, 1977); Flandrin, "Sentiments et civilisation: Sondage au niveau des titres d'ouvrages," *Annales*, 20 (1965): 939–66, and *Les Amours paysans* (Paris, 1978); and Hémardinquer, ed., *Pour une histoire de l'alimentation* (Paris, 1967), and ed. with M. Keul and W. G. L. Randles, *Atlas des plantes vivrières* (Paris, 1975). For bibliographic guides to the rich historiographical work of *Annales* on themes of "material civilization," see André Burguière, "L'Anthropologie historique," Roger Chartier, "Outillage mental," Jean-Marie Pesez, "L'Histoire de la culture matérielle," and Michel Vovelle, "L'Histoire et la longue durée," in Chartier et al., *La Nouvelle Histoire*.

to subject launches the reader toward the past with marvelous élan. Braudel's historical account is like the Mediterranean: a basin full of shifting, sun-lit water, scintillating with varied and variable insights, ever moving swiftly on.

BUT WHAT THEN? It is the surface of the text that fascinates, not its depths. *La Méditerranée* itself has been, and remains, rather more of an "event" than a "structure."

Reviews of Books

GENERAL

G. W. TROMPF. *The Idea of Historical Recurrence in Western Thought: From Antiquity to the Reformation*. Berkeley and Los Angeles: University of California Press. 1979. Pp. x, 381. \$20.00.

Readers should take G. W. Trompf at his word. This book is indeed about an idea (or rather a "family" of ideas); it is not about a theory. Correspondingly, it discusses particular passages in which historians employed metaphors or similes of recurrence to ornament, advance, and explain their broad narrative strategies. Philosophy and theology appear in sections that Trompf provided for context, but the book as a whole is not about systems of formal thought in which ideas of recurrence belong to broad, coherent theories of knowledge or existence.

Trompf has written about an arrow in the historian's quiver instead of about the art of hunting. Radical shifts in *mentalité*—and, to be sure, in the audiences and genres of historical writing—do not impinge on the account. Trompf holds that the range of variations on the theme of recurrence was already complete in Polybius (to whom he devoted two of his five chapters). Identifying this quite miscellaneous repertoire is a major achievement of the book. In his later chapters, Trompf considers passages in which one or another of those variations figured in historical texts up to the mid-sixteenth century. He does not, however, suggest that the "recurrence paradigms" multiplied or experienced elaboration in the sixteen hundred years between the heyday of republican Rome and the outbreak of the Reformation.

Trompf's focus on the individual element, rather than on the whole system, produces an illusion of timelessness regarding the "paradigms" of recurrence, but it also opens, secondarily, perspectives on the mechanics of cultural assimilation. One of Trompf's main objectives is to demolish the familiar, simplistic contrast between Greco-Roman (cyclical) and Hebraic (rectilinear) concepts of historical progression. He defends his position by arguing that every historical writer employs an assortment

of "paradigms," a "cluster of interpretive frames," some of which may be incongruous. Thus, he maintains, an author could combine cyclical and rectilinear paradigms. Although one might predominate for him, there were, in the rich and possibly inconsistent texture of his thinking, alternatives that could resonate when his culture came in contact with a foreign one. As a primary example, Trompf discusses the author of the Gospel of Luke (identified also as the author of Acts) who, he holds, was able to assimilate "recurrence paradigms" in Gentile historiography with Old Testament themes of restoration, models of good and bad conduct, and promises of moral retribution.

No doubt such parallelisms do play roles in cultural assimilation. And yet parallel figures of speech, or even of thought, in two cultures do not necessarily indicate analogs in the deep and complex patterns of collective *mentalité* or in the elaborate interchanges that render the texts of a given writer intelligible in the singular light of his own age. When Trompf decided to pursue an idea instead of a theory, he also decided to leave aside integrating cultural assumptions as well as the broad metaphysical or theological commitments that made the network of "recurrence paradigms" cohere for the individual historians whom he discusses. For this reason, his discussion of Luke-Acts seems tentative, rather than conclusive.

In brief, his book sets forth the "paradigms" as though they were rhetorical *topoi*—grounded, to be sure, in common human needs and experiences but formalized in the schools of classical antiquity and awakened from an arcane half-life in the Renaissance. If the book is considered a study in rhetorical *topoi* and, more broadly, in history as an enterprise colored by classical rhetoric, one can understand how Trompf felt able to give ancient writers (especially Polybius) a distinct predominance over others in the long period reviewed, to discuss the Gospel of Luke (and the book of Acts) without reference to theological content, and to pass over the wealth of patristic and medieval historical writings in a cursory fashion. This perspective raises serious questions for anyone concerned to know whether the recurrence paradigms were isolated, random figures

of speech or signs of coherent ways of patterning human experience, and, moreover, to understand what mechanisms sustained the vivacity of the classical tradition as it passed down through highly diverse cultures. For the moment, Trompf has deferred addressing those questions, but he will need to confront them as he goes about accomplishing his plan to study the idea of recurrence from Vico to Arnold Toynbee.

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KARL JOACHIM WEINTRAUB. *The Value of the Individual: Self and Circumstance in Autobiography*. Chicago: University of Chicago Press. 1978. Pp. xix, 439. \$19.00.

This is a thoughtful essay on "the emergence of individuality in autobiographic writings from Augustine to Goethe." Karl Joachim Weintraub begins with a discussion of St. Augustine's *Confessions*, a work that "stands out with such prominence in proportion to everything preceding it that it is difficult to resist the assertion it is here that the true autobiographic tradition of the Western world took off" (p. 17). A short description of Augustine's world and of his life is followed by a longer analysis of his attempts to answer the important questions: "Who am I? How did I get to be what I am? How can I save my soul?"

The same pattern—historical sketches of the life and times of an individual autobiographer followed by an analysis of how he or she answered the important questions about the relation of the self to the world outside—is used in discussing fifteen other writers who contributed to the development of the modern idea of personality. The fifteen are Abelard, Petrarch, Cellini, Cardano, Montaigne, three mystics (Seuse, St. Teresa, Madame Guyon), three Puritans (Bunyan, Baxter, Franklin), Vico, Gibbon, Rousseau, and Goethe.

By the time of Goethe, historicism had triumphed: "a fascination with diversity," "a sense of the unique value of each moment," and "a genetic mode of understanding human reality." Observing that "Nature, the All resting in God's lap, seemed forever to be fashioning individuality in her grand workshop," Goethe concluded that it was a natural right to value and cultivate one's own individuality (pp. 368, 370). He realized, however, as Rousseau did not, that assigning so high a value to the self can lead to a dangerous egocentrism. Anyone can easily misuse the right to be himself: "doing one's own thing" is a modern vulgarization of a fruitful idea. The self must relate to something outside. "Augustine had learned to find his soul by surrender to God"; "Goethe discovered the same wisdom in relation to a historical social world and a pan-

theistic conception of ever-evolving Nature" (p. 376). *Dichtung und Wahrheit* "represents the moment in the history of autobiography when the self-understanding . . . of an individual parallels the emerging historicist mode of understanding human life" (p. 368).

The outlook of historicism and individuality—a loving admiration for the diversity and richness of life—is a grand one. But, Weintraub remarks, we pay a high price for it: we may easily flounder in a capricious subjectivism and in unrealistic utopian dreams unless we can hold fast to the steady interaction between the self and our own historical, social, and cultural world. "Only the future can show whether the price is too high and whether we can live responsibly with such an ideal of the self" (p. 379).

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ALFRED BRAUNTHAL. *Salvation and the Perfect Society: The Eternal Quest*. Amherst: University of Massachusetts Press. 1979. Pp. xiii, 433. \$25.00.

In this work Alfred Braunthal takes for his subject "the history of both the quest for salvation and for a perfect society" (p. ix). He aims to examine the history of these themes, the relationship between them, and the various changes in that relationship over the centuries of human history. The quest for salvation is religious, while the quest for the perfect society is secular. It is the author's view that sometimes these quests are opposed and separate, but sometimes they converge and influence each other.

The book is divided into ten chapters and begins with a survey of ancient societies whose view of salvation meant "protection against material evils" and "success in the pursuit of material strivings" (p. 1). Braunthal gives primary attention here to ancient Judaism, which, along with Christianity and Islam, is most characteristic of the union between the quest for salvation and the perfection of society. Nor are the Eastern religions ignored: chapter 2 traces the theme of salvation from suffering and from rebirth in the Vedas and Upanishads, the various varieties of Buddhism, and in Confucianism and Taoism.

The author then considers the development of Christianity, its relationship to Greek dualist doctrines, mystery cults, and apocalyptic currents in late Judaism, the role of the medieval church as the primary agent of salvation, and the salvational implications of the Reformation, moderate and radical. We then move on to modern trends in Christian beliefs, a glimpse of the world of Islam, and, in the two concluding chapters, a survey of the role of secular humanists, who from the seventeenth to the

nineteenth century "based their faith in the perfectibility of human society on two doctrines—the doctrine of man's natural rights and the doctrine of a law of progress" (p. 277)—but in the twentieth century have encountered problems that test the courage and convictions of religious and secular humanists alike.

The scope of the book is clearly wide ranging, which leads inevitably to a rather superficial look at much of the material. The book also appears to have a nonspecialist audience in mind. Explanatory material in the text and in the notes, as well as a rather general glossary of proper names, supports this view. More serious, however, is the level of documentation in the work. There is no separate bibliography, and a close look at the footnotes shows excessive reliance on secondary authorities. Surely the intelligent lay audience deserves more in the way of sources than encyclopedia articles—a frequent reference.

What then can we say about this book? Braunschweig obviously shows an enthusiastic interest in his subject and has taken us over the long history of the human hope for both other-worldly salvation and for a more perfect society here on earth. His essay raises optimistic hopes for the continuing quest, but as intellectual history it falls far short of the mark.

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WALTER HORN and ERNEST BORN. *The Plan of St. Gall: A Study of the Architecture and Economy of, and Life in a Paradigmatic Carolingian Monastery*. With contributions by WOLFGANG BRAUNFELS, CHARLES W. JONES, and A. HUNTER DUPREE. In three volumes. Berkeley and Los Angeles: University of California Press. 1979. Pp. xxviii, 356; xii, 359; xxxiv, 267. \$325.00.

The original plan of St. Gall was made at the island abbey of Reichenau between 820 and 830, and a copy of it shortly afterwards was sent to the monastery of St. Gall near Basel. Preserved accidentally when a twelfth-century monk wrote a life of St. Martin on the reverse, its significance was not realized until 1604. It is now a national treasure of Switzerland. The plan was designed for a self-supporting monastic community of 270; all of the buildings were functional and closely integrated one to another and to the great basilican church. Although no monastery was ever built from it, the plan of St. Gall was intended to serve as a utopian model of monastic architecture and organization for all of Carolingian Europe. As such, it is one of the great documents of Western culture and a work of art in its own right. To its study, two scholars at Berkeley, Walter Horn, an art historian, and Ernest

Born, an architect, have devoted twenty years and have tried in these books "to comprehend the whole life of the people for which these buildings were planned" (vol. 2, p. 2).

Volume 1 includes the vast scholarly literature on the plan; a historical and architectural discussion of the church, cloisters, and abbot's house; and a good deal of early monastic history and social, manorial, and intellectual organization. Volume 2 provides an extensive treatment of guest and service buildings and of the influence of the plan on later medieval building; this is probably the most original part of the entire work. Volume 3 contains the appendixes, with a catalogue of the inscriptions on the plan; an elegant translation of the *Customs* of the Abbey of Corbie; and a section on "The Significance of the Plan of St. Gall to the History of Measurement." It concludes with a lengthy Chronological Table and an almost exhaustive index.

These volumes are, first, a stunning success in graphic design. With easily readable photographs, maps, and illustrations—one thousand in color; with 95-pound paper and nonglare ink; with the footnotes on the same page as the text for ideal cross-referencing; with lengthy captions and animated designs throughout; and utilizing the latest methods of printing technology, they constitute a great and beautiful artistic achievement and a milestone in the history of the book.

The erudition represented here is encyclopedic. The authors have used the evidence of art and architecture, archeology, maps, aerial photography, place names, paleography, and of all kinds of literature. Although especially strong on the construction of vernacular (that is, nonecclesiastical) buildings, the area of medieval architectural history where relatively little has been done, these books provide a rich compendium of information on many facets of ancient and Carolingian life and culture: religion and monasticism, obviously, but also handwriting and book making; stoves, ovens, and methods of heating and cooking; kinds of medical therapy; systems of horticulture and agriculture; the manufacture of barrels and all sorts of utensils; the production of beer and wines; drainage, sewage, and even the material needed in the privies (straw). Prescribing facilities and techniques that were highly advanced for its time, the plan of St. Gall, for example, provided for more toilets per person than does the present sanitary code of the U.S. Army. These volumes are as brilliant a paradigm of "total history" as we are likely to see, and students of many facets of medieval culture will want to consult them. Costs obviously will restrict purchase to libraries.

In this monumental and virtually definitive achievement, Horn and Born have given us a new appreciation of the meaning of Western humanism.

They have also given us a fresh insight into the ancient question of the psalmist: "What is man that you are mindful of him? . . . You have made him a little less than the angels."

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ISADORE TWERSKY. *Introduction to the Code of Maimonides* (Mishneh Torah). (Yale Judaica Series, number 22.) New Haven: Yale University Press. 1980. Pp. xvi, 641. \$40.00.

Maimonides' code is one of the greatest and most influential works in Jewish history, and Isadore Twersky's long-awaited study is worthy of its subject. The book begins with a discussion of Maimonides' motives in composing the *Mishneh Torah* and then analyzes its form and scope, its novel system of classification, its linguistic features, and the relationship between law and philosophy both in the code itself and in the Maimonidean corpus in general.

Since Twersky has illuminated these issues through an examination not only of Maimonides' predecessors but also of his successors, and since the *Mishneh Torah* had an impact on virtually all of subsequent rabbinic literature, the erudition reflected in this book is staggering. Material that could generate entire articles is compressed into single footnotes, while the discussions of topics accorded more expansive treatment virtually constitute definitive monographs (for example, the section on reasons for the commandments in chapter 6).

Twersky's wide-ranging discussion of Maimonides' motives assumes that the varying Maimonidean statements on this subject all reflect genuine elements in the author's psyche. The most problematic point in this context involves Maimonides' attitude toward the continuing study of the Talmud. Twersky argues forcefully that, although Maimonides preferred the codificatory form, he was continuously engaged in talmudic study and actually expanded its scope by paying attention to subject matter that had been widely neglected in his milieu. Despite the explicit and candid discussion of this problem on pages 210-12, the reader, who is confronted with the actual text of the statement defending talmudic study in Maimonides' letter to Rabbi Phineas of Alexandria, would have been better served if the rather different comments in the letter to Rabbi Joseph b. Judah had also been presented in a direct quotation.

In a chapter that addresses the essence of the code's achievement, Twersky convincingly elucidates the reasons for some of the difficult decisions determining the classification of law in the *Mishneh*

Torah. I think, nevertheless, that Maimonides remains vulnerable to the judgment that he relied on conceptual tours de force to effect an excessive reduction in the number of his categories.

Twersky's appreciative and insightful analysis of Maimonidean style is mirrored in his own elegant literary flourishes. This tendency can lead to overwriting; it hardly seems necessary, for example, to explain that divergent positions on law tend to be "theoretical-interpretative" while discussions of variant customs are "empirical-consuetudinary" (p. 124). On the other hand, I have never seen a better piece of writing in a scholarly study than the passionate, magnificent peroration on the unity of law and philosophy in Maimonides' works (pp. 507-09).

Twersky's position on this vexed question rejects the bifurcation of Maimonides into a philosopher in his *Guide* and a jurist in his code; the code itself provides ample evidence of Maimonides' rationalistic, although genuinely religious, stance. Despite the length and quality of the chapter on law and philosophy, there are many problems relevant to this question that Twersky can only note in passing. The entire book, in fact, is studded with issues that cry out for elaboration, and the preface promises a companion volume in Hebrew that will fill at least part of the gap. The reader finishes this remarkable 537-page study with the desire to read another book about the *Mishneh Torah*—and with the hope that Isadore Twersky will write that book.

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IAN MACLEAN. *The Renaissance Notion of Woman: A Study in the Fortunes of Scholasticism and Medical Science in European Intellectual Life*. (Cambridge Monographs on the History of Medicine.) New York: Cambridge University Press. 1980. Pp. vii, 119. \$16.50.

This condensed essay, eighty-nine pages in length, reviews the major European theological, medical, political, and legal literature on women from the fourteenth century to the middle of the sixteenth century. Its compression derives in part from the adoption of the classical treatise as literary vehicle, right down to enumeration of chapters and paragraphs. Vocabulary mirrors that literary device faithfully; it relies heavily on Latin constructions. Clarity and brevity are served, but one wonders whether we still produce enough classically prepared minds to appreciate Ian Maclean's virtuosity.

It would be a shame if this essay were to miss the audience for which it is intended because of its erudition. The weight of its scholarship is brought to bear on investigating what is genuinely notional in

defining woman, that is, the accumulated commonplaces of medieval and Renaissance thought that were shared from discipline to discipline. Maclean does not lapse into reverence for ancient authority, equating antiquity with rectitude, but critically pursues the reasons why the restraints on the conceptualization of the notion of woman remained so long in force despite some new currents of thought: neoplatonism, physiological investigation, an emerging but still frail historicism in the study of law, and a small corpus of feminist writing. He concludes, in restrained fashion, that the deeply felt desire for synthesis in scholastic thought prompted a conservative discourse of a pluridisciplinary nature. This discourse relied upon shared pagan and Judeo-Christian assumptions about the inferiority and lower status of women. The author asserts that the concordance between these two notions disguises their differences, a point that he could well have granted more discussion, even at the risk of lengthening his essay. His second conclusion is that the well-established institution of matrimony impeded advance, since thought on matrimony continued to employ the Pythagorean devices of contraries and opposites to the detriment of women's autonomy, even humanity. The author refuses to consider social and economic issues and their influence on notions of women's place, as well he might in a brief study of scholastic and humanist thought, but in a certain sense he does enter the realm of social change. He notes that one of the few effective devices employed by intellectuals critical of prevailing thought was the "joke" or satire. Both relied for their impact on the contrast between strict attention to the classical argument and the reality of daily experience. For this device to succeed, contemporary women's roles must have given the lie to the inherited canon of ideas.

This study sets precise limits on its inquiry and should be judged on that basis. For those of us who have given the greater part of our attention to the documents of practice rather than the prescriptive literature, Maclean's brief essay provides an appreciated balance. His intent, borne out of his earlier study of seventeenth-century French feminist writing, is the best indicator of his success. He set out to test the intellectual fortunes of the notion of woman inherited by Renaissance thinkers. He has briefly and lucidly identified the constraints on change to that very limited concept.

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MICHAEL ERMARTH. *Wilhelm Dilthey: The Critique of Historical Reason*. Chicago: University of Chicago Press. 1978. Pp. xiv, 414. \$19.00.

This absorbing revision of Wilhelm Dilthey's reputation will generate discussion among intellectual historians because it goes against much of the conventional wisdom on the subject. But it is not likely that Michael Ermarth's study will soon be surpassed in scholarship or plausibility of interpretation.

The Anglo-American world first became acquainted with the abstruse German thinker largely through the pioneering works of H. A. Hodges. Up to the present, however, it has been difficult to come to grips with the seemingly Protean nature of his philosophy, no less difficult than the proverbial means of capturing a bird by putting salt on its tail. First you must get it to sit still, and the probing mind of Dilthey was always in motion. Despite José Ortega y Gasset's well-known Dilthey essay in *Concord and Liberty* (1946) that characterized him as simply the most important thinker in the second half of the nineteenth century, he is still often dismissed as the "great fragmentist" who embodied the "anarchy of convictions" he himself pronounced as a diagnosis of the moral confusion rife in turn-of-the-century Europe. Nothing published on him in German or English since his rediscovery after World War II has done justice to the shifting multiple layers and nuances of his evolving "critique of historical reason." Ermarth has come very close indeed, and the appearance of his book is a notable event in Dilthey scholarship.

Ermarth's stated aim is to understand Dilthey in his own terms. He draws his scholarly wagons into a hermeneutical circle and defends Dilthey against the charges of those who have seen him variously and simplistically as a positivist, a neo-idealist, a relativist, a historicist, a votary of irrational vitalism (*Lebensphilosophie*), or merely as a bridge between Wilhelmine thought and the modernist philosophies identified with Husserl and Heidegger. Between those who find a strong continuity running through Dilthey's works and those who detect a drastic change in the late 1890s, he takes a middle position: there was significant modification and deepening of key ideas along with a persistent focus on a complex insight contained in the term "lived experience." Making illuminating use of unpublished material found in the Dilthey *Nachlass*, Ermarth argues that this concept (*Erlebnis*) and related ideas were neither wholly subjective in their meaning nor vitalistic in the biological sense.

The author rightly refuses to place Dilthey under any conventional rubric. Preferring to describe him as a sophisticated representative of *Idealrealismus*, he traces this tradition back to F. D. Schleiermacher and to Dilthey's teacher A. Trendelenburg. *Idealrealismus* is described as "a realism which could not be converted into materialism or naturalism be-

cause it accepted mind as real along with sensible objects; an idealism which could not become subjectivism because it held that mind must always follow the binding evidence of what is given in experience" (p. 344). As an ideal-realist, Dilthey naturally first looked to psychology as the discipline that might produce a basic mental science for the human studies by combining rigorous empirical methods with due recognition of the dynamic reciprocity between mind and historical culture. Although Dilthey never entirely gave up his belief in some kind of psychological basis for historical understanding (*Verstehen*), he became more and more disappointed in contemporary psychological theories because he found them mechanistic and reductionist. After his encounter with Husserl's *Logical Investigations* (1900–01), he came to recognize "the intentional-objective and the historical-cultural nature of mental life . . ." (p. 216). The earlier notion of "lived experience" is still the crimson thread, but Dilthey has changed his emphasis "from the engendering acts to the resultant contents" (p. 219).

With his distinction between "lived experience" and sensory experience, and with related categories such as "reflected experience," "expressions," "objective mind," "life-values," and "world-views," Dilthey was assaying nothing less than a philosophical revolution, an overthrow of such misleading hypostatizations as the Cartesian *cogito* and the Kantian transcendental ego, a rebellion anticipated earlier in different ways by such holistic historical thinkers as Herder and Hegel.

Ermarth's spirited defense of his subject almost carries him across the fine line between forceful scholarly argument and partisanship. Fortunately the conclusion brings him back from the brink by offering a cool analysis of strengths and weaknesses. Dilthey's reach, as it turns out, did exceed his grasp, and his noble dream of a foundational science for the humanities remained unfulfilled. Yet as Dilthey himself was a philosopher of process, his own career should be followed in the spirit of open-mindedness and fluidity rather than be motivated by a futile hankering after systematic coherence. If Dilthey's searchings were incomplete and sometimes obscure, they nonetheless come close to what many humanists and social scientists are actually doing today, even if unaware of their Diltheyan heritage. Ermarth's excellent book reminds us that the ancient feud between humanists and social scientists has been allowed too long to distract us from the common task. Dilthey himself described that task as advancing "the capacity of man to know himself and the society and history which he has created" (quoted by Ermarth, page 93). There has been much written in recent years about the convergence of history and the social sciences. Ermarth's superb

exegesis of Dilthey suggests that were he alive today he would be leading the parade.

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RONALD HAYMAN. *Nietzsche: A Critical Life*. New York: Oxford University Press. 1980. Pp. xxiii, 424. \$19.95.

Like his two big *bêtes noires*, Socrates and Jesus, Friedrich Nietzsche saw his life and his teachings as inseparable. He insisted that a philosophy must be lived to be true—intimating, falsely, that he was living by his own Dionysian creed. More, he declared every philosophy to be a personal confession (after Lou Salomé) and kept dropping hints about the self-referencing in his works. Appropriately, he documented his life to the full. He reported his occupations and preoccupations, and especially his ailments, to a dozen or more correspondents in any given season. He also kept journals and wrote autobiographies between times.

Scholarship has hardly begun to meet this biographical challenge. Ronald Hayman's *Nietzsche* is page for page as accurate, insightful, and readable as any of its predecessors, yet it too leaves the key questions about Nietzsche's development unanswered. How did that pious Lutheran lad ever turn into God's necrologist (and self-appointed successor)? Here Darwinism was decisive intellectually, but Hayman notes young Nietzsche's "Darwinist doubts" only to introduce a quotation expressing them (p. 44). How did Nietzsche's goody-goody "Naumburg virtue" metamorphose into immoralism? Why did that Prussian patriot go antinational and Germanophobic? By what magic did a sometime heavy-handed academic forge that trenchant, vibrant, exciting literary style? Once when he was twenty-two he tried to write well, then told a friend: "I could not do it, and I was angry" (p. 85). Hayman's Nietzsche loses his original calling by the way: "No longer intending to go into the Church" (p. 49). He chooses philology over music with as little cause. His music itself is never discussed. In the human, all-too-human realm, Hayman points up Nietzsche's sadomasochism without probing it. He also chronicles Nietzsche's medley of symptoms without making much of them. Even so, his account of Nietzsche's last decade, when all was sickness, is moving: there his sober closeness to his sources pays off.

Nietzsche finds are always welcome, and Hayman's include Richard Wagner's resemblance to Nietzsche's father and the influence of Carl Spitteler's *Prometheus und Epimetheus* on Nietzsche's *Zarathustra*. Without taking any discernibly fresh look at

Nietzsche's thought (despite the book jacket), Hayman does show a firm grasp of Nietzsche's meaning and originality. I have no worse quarrel on the one score than that Nietzsche did not glorify "master morality" (he just expressed it gloriously) or on the other than that Nietzsche's devaluation of the will to truth in *Beyond Good and Evil* was hardly ahead of its day (witness Ibsen's in *The Wild Duck*). Two final complaints: Hayman's source references are inadequate, while his comparative references to an Arthur Rimbaud or a Marquis de Sade verge on name-dropping. But these failings detract little from a uniquely balanced and faithful version of a life that just calls for so much more.

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L. J. RATHER. *The Dream of Self-Destruction: Wagner's Ring and the Modern World*. Baton Rouge: Louisiana State University Press. 1979. Pp. xx, 215. \$17.50.

One of the intriguing things about work on Richard Wagner is the variety of scholars and essayists involved in it. L. J. Rather, a professor emeritus at the Stanford University School of Medicine, has written a number of books on the intellectual history of medicine and, clearly having a Wagnerian bent, has done an absorbing study of the composer's ideas. He brings to the subject much erudition on nineteenth-century thought and letters and shows considerable finesse in viewing Wagner from that perspective. Although not entirely convincing in all its arguments, the book should interest cultural historians on a lot of counts.

Its title suggests its thesis: the development in Wagner's works of a dream of self-destruction, a vision of approaching cataclysm in both society and the individual psyche. Working through the basics of Schopenhauer's thought in some detail, Rather argues that Schopenhauer provided Wagner a language of cultural pessimism that struck receptive chords in such figures as Thomas Carlyle, H. S. Chamberlain, and G. B. Shaw. The discussion of the evolution of the *Ring's* libretto is particularly useful, showing the growth of the idea of destruction in Wagner's thinking. Although George Windell's 1976 article in *Central European History* on Wagner, Schopenhauer, and Feuerbach probably has more value on that subject, Rather contributes a broad perspective of the many kinds of other figures who were related to Wagner's ideas. He makes the case that the *Ring* is "the supreme artistic expression of a nightmare that began increasingly to haunt the conscience of Europe early in the nineteenth century—the dream of a day of judgment lying in wait for a race that had lost touch with the source of truly human life in its frenzied quest for gold and

power without end" (p. 181). The idea, Rather argues, amounted to "a peculiar nineteenth-century pattern of mythic correspondences" (p. xx).

But "myth" is a slippery word, and Rather has not defined it overly well, calling his technique "montage, constructionist, or documentary" (p. xx) and leaning too heavily upon hindsight from the trauma of World War I. Although the stresses of social change may have made the idea of destruction naturally popular on many fronts, it is not altogether clear just what it amounted to. And that aside, was Wagner a leading nineteenth-century prophet, as Rather seems to think? One comes away from the book not completely convinced, impressed instead mostly with how skillfully Wagner fastened onto so many trendy ideas with which to push his music dramas. Rather claims that Wagner anticipated certain ideas of Freud, primarily the transformation of the unconscious into the conscious. Although he proves full well how central dreams and sexual themes are in Wagner's prose, it is hard to see him more than vaguely as a psychological thinker. This kind of intellectual history has distinct limits when applied to a man of the world—a theatrical entrepreneur—such as Wagner. Rather also gets more involved in the wrangle over Wagner's antisemitism than is pertinent to the book. Still, he offers some rich and provocative ideas that deserve close attention.

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FRANK J. SULLOWAY. *Freud, Biologist of the Mind: Beyond the Psychoanalytic Legend*. New York: Basic Books. 1979. Pp. xxvi, 612. \$20.00.

Trained as a historian of science, Frank J. Sulloway wanted to write "a comprehensive intellectual biography of Freud" and "bring both Freud and the history of psychoanalysis within the professional boundaries of the history of science" (p. xiii). He asserts that many of Freud's ideas and modes of thought represented transformations of conceptions available in late nineteenth-century biology. Freud's closest friend, the physician Wilhelm Fliess, and sexologists such as Krafft-Ebing and Albert Moll showed Freud that biology offered greater flexibility in understanding and describing the mind than the psychophysics of the "Helmholtz school" usually emphasized by students of Freud.

It turns out, but only after a reader is far advanced into this book, that Freud used a biology consisting mostly of premises and presuppositions rather than of "hard" scientific findings. For instance, he often resorted to Ernst Haeckel's "biogenetic law" that a developing organism will retrace

patterns of development undergone by its ancestors: "ontogeny recapitulates phylogeny."

Usually a biologist would apply Haeckel's findings in embryological investigations and produce a neat and limited explanation, one that would be "proximate-causal" in Ernst Mayr's distinction. Mayr went on to point out that biology also invited and offered "universal-causal" explanations: these would be evolutionary and historical. According to Sulloway, Freud proceeded to turn from the largely proximate neuroanatomical modes in which he had been trained to the grander vistas of Darwinism.

Although Sulloway dedicated the book to Mayr, he simply drops Mayr's analysis into the text long after the book is well under way, amplifying it a bit later in a few places. Similarly, Sulloway likes I. Bernard Cohen's idea that a great figure in science transforms already existing materials, but Sulloway invokes or alludes to this concept far more than he expounds it systematically. Skimping on theoretical structure in a very long book heightens the impression that Freud mostly borrowed or took a good deal from others; it fails to clarify why he borrowed or how he reshaped what he took over.

Freud's use of the double nature of biology forms the real center of the book. Freud learned from Fliess to make large-scale biological guesses, hypotheses, and assumptions. Thus Freud maintained that, because of Haeckel's law, the behavior of modern children and adults must repeat the experience of humanity dating back to the Ice Age and beyond. This generalization of a finding from embryology was unwarranted but enticing and enthralling.

No previous student of Freud has demonstrated the pervasive biological content of essential Freudian notions. Indeed, Sulloway first had to recover much of what he analyzes, because Freud's biology appeared in psychoanalysis mostly as masked or "crypto-biology." Freud de-emphasized his biological debt, Sulloway argues, because he was wedded to the myth that he had created psychoanalysis in total isolation, owing nothing to anyone, and because he pushed a view of psychoanalysis as pure psychology in order to beat off and discredit biologizing innovators such as Alfred Adler and Wilhelm Stekel.

On some issues, such as why Freud posited the death instinct, Sulloway offers the best interpretation I have seen. On others, his unweaver's zeal occasionally leads him into unnecessarily all-or-nothing formulations. Legions of Freud biographers have stressed the centrality of Freud's self-analysis in demolishing his conviction that neurosis resulted from parents or other adults actually seducing children. The unanimity of these pre-Sulloway writers need not, however, mean that the self-analysis played no role at all. The letters to Fliess suggest that abandoning belief in seduction took Freud a

long time, and for at least a part of that time he was occupied with analyzing himself as a patient and discovering the power of fantasy, as opposed to the effect of events that had really occurred.

Sulloway's commitment to disclosing the sources of Freud's ideas leads him to pay scant attention to the fact that only Freud created psychoanalysis. Fliess did not, and the sexologists did not. Sulloway ends his book with a "Catalogue of Major Freud Myths" and rebuts each. The reader finds himself left with the feeling that there was not too much for Freud to do once he had taken over so many valuable insights and concepts from biology. Or was there? What, after all, did he really do?

Freud did not discover the unconscious. He created a language for talking about it at length. A significant part of this language came from biology because biology consisted of a storehouse of knowledge about, and ways of representing, that which was unconscious and took place outside the conscious awareness of every organism, including man. Sulloway shows Freud using biology, but he does not make clear this peculiarly deep usefulness.

Freud could teach his language to others and enforce their use of it. Because he fashioned a language he went far beyond the accomplishments of poets and philosophers who never managed to say nearly as much about the workings of the unconscious. In fabricating this language Freud mixed many sources, fused them, and turned from one to another as circumstances demanded, just as Picasso from the 1930s on moved freely among many styles, none of which he abandoned.

Sulloway has increased significantly our understanding of Freud by identifying some critical building blocks, and he may well have found one of several cornerstones. What Sulloway calls "myth" I regard as the central historical phenomenon still unexplained and consistently passed over in this book. Freud borrowed, absorbed, and pilfered from others, but only he established psychoanalysis. How did he, by himself, erect that structure which has appeared, in its fullness, just once in Western history?

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MICHAEL ERBE. *Zur neueren französischen Sozialgeschichtsforschung: Die Gruppe um die "Annales."* (Erträge der Forschung, number 110.) Darmstadt: Wissenschaftliche Buchgesellschaft. 1979. Pp. xii, 159.

The *Annales* group in the last three decades has received a relatively critical reaction in West Germany. Both conservative historians (for example, Gerhard Ritter) and historians writing from the perspective of "Critical Theory" (for example, Die-

ter Groh) have accused the *Annales* of methodological dogmatism and naturalistic, ahistorical reductionism. In part these criticisms rested on a general lack of information. Few of the important works of the *Annales* group, not even its classics, were translated into German, and only in 1977 did a small Suhrkamp paperback appear with translations of representative articles from the journal. The new interest in historical demography and a broadly conceived social history of everyday life in pre- or protoindustrial societies has led to an awareness of main directions of French social history and to incipient institutional contacts.

Michael Erbe's study is by no means an exhaustive history or analysis of the *Annales*. Nor is it meant to be. A study that assesses the place of the *Annales* in modern social history still needs to be written. Troian Stoianovich largely restricted himself to an examination of certain approaches since 1968. Erbe seeks to survey the *Annales* approach since its beginnings.

This little volume forms a part of a series, "Erträge der Forschung," on tendencies in recent scientific and scholarly work. Within the confines of the series, Erbe's book fulfills its function very well. It is lucid and informative, perhaps too lucid in dealing with the more recent anthropological contributions to the *Annales*. A strength of Erbe's volume, in fact, is that he has no special theses to defend; his presentation is remarkably value-free—in contrast to much of the German literature, with the notable exception of Claudia Honegger's excellent introduction to the Suhrkamp anthology, *Schrift und Materie der Geschichte* (1977). The one point that Erbe stresses consistently and rightly is the methodological openness, the *Methodenpluralismus*, of the *Annales* group that has made it possible for them to open so many new areas of historical study and, in particular, to lay the foundations for a "history of the average human being and his everyday" life (p. 136). The Germans in Erbe's opinion thus have a lot to learn from the *Annales*, but the *Annales* historians, he stresses, must overcome their neglect of politics if they wish to approach history in its entirety.

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JUDITH A. SABROSKY. *From Rationality to Liberation: The Evolution of Feminist Ideology*. (Contributions in Political Science, number 32.) Westport, Conn.: Greenwood Press. 1979. Pp. viii, 175. \$17.95.

So untouched by scholarly attention is the field of the history of feminist theory that one would expect any contribution to be useful. But even in this

scholarly desert, this book's reliability is so questionable that one cannot recommend it.

Judith A. Sabrosky claims to survey feminist ideology (treated as a homogeneous belief system) since the eighteenth century. In fact she comments on the work of some individuals chosen with no apparent logic, nor with any explanation offered: Mary Wollstonecraft, Frances Wright, Flora Tristan, Sarah Grimke, Margaret Fuller, Elizabeth Oakes Smith, Harriet Taylor Mill, Caroline Dall, Simone de Beauvoir, Betty Friedan, Germaine Greer, and Elizabeth Janeway. It would be hard to imagine a more capricious list. Among the excluded are John Stuart Mill, William Thompson, Friedrich Engels and all Marxists, Elizabeth Cady Stanton and all the nineteenth-century American suffragists, the Vaertings and all German feminists, to name a few; Charlotte Perkins Gilman, the most important feminist theoretician of the nineteenth century, is barely mentioned; the contemporary feminist movement is represented by media personalities and all its serious theoreticians are omitted.

Although Sabrosky begins with a definition of ideology, the definition (she calls it "composite") is so loose it creates no skeleton for the body of the work. In the rest of the book there is no operative distinction between "ideology," "theory," "philosophy," or just "ideas."

The summaries of individual feminist writings contain many errors. Margaret Fuller is the author's favorite, and we are told that Fuller was the "first ideologist to demonstrate concern for woman as woman." This is an absurd claim, and it is followed by an equally false argument: that Fuller's view that woman is "intuitive and, consequently, superior," that woman "must control society because of her greater potential for inspiration and intuition," formed "the philosophical bases of both nineteenth- and twentieth-century feminist ideology." A great deal of feminist writing since Fuller was in fact devoted to denying the uniqueness of woman at all. Sabrosky incorrectly attributes to Harriet Taylor Mill a view of woman's nature as impulsive and instinctual. She claims that "innovations" in feminist ideology ceased after 1850 and writes that feminist analyses after that were restricted to the suffrage issue. Any textbook on the woman movement could correct these and many other errors.

Virtually none of the central issues of feminist ideology of the last two centuries are examined in this book. There is nothing on the question of how gender is constituted and maintained, nothing on the origins and maintenance of women's subordination. Nothing about the ideals of female liberation—equality with men? A respected junior partnership? The sexual division of labor, and the question whether its abolition is necessary to sexual equality, is never mentioned. The debate between

those feminists who called for androgyny and those who celebrated female uniqueness is not recognized.

Indeed, it is difficult to see what is being discussed due to the vagueness and imprecision of the ideas expressed. For example, we learn that Grimke's strategy was "woman achieving her own equality by asserting herself"; and that "strategy" is opposed to Wollstonecraft's call for rational education and to the "socialist-feminist" (by which she means Wright and Tristan, presumably) call for socialism.

Part of the problem of this book is that it is impossible at this time to do an intellectual history of feminism on the basis of published sources. Sabrosky's mistakes may be honest ones resulting from lack of familiarity with women's social history and unpublished manuscripts. But there is an illogic in the book, too, as is apparent above in opposing women's self-assertion to demands for education and/or socialism.

In the conclusion the illogic also takes on the form of a peculiar argument, or perhaps just an undigested observation: that ideology precedes activism. "Wollstonecraft prescribed an equal education . . . before women demanded [it]. . . . Beauvoir contended that women should assume traditionally exclusive male roles . . . before women entered the labor force in significant numbers." Another incorrect statement, of course, particularly for France where by 1856 nearly one-half of all women were doing paid labor, by 1911 over one-third. By detaching feminist ideology so completely from traditional social theory, the feminist social movement, or the actual conditions of women's lives, the author attempts to justify a great deal of omission and distortion.

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ALFRED GROSSER. *The Western Alliance: European-American Relations Since 1945*. Translated by MICHAEL SHAW. Foreword by STANLEY HOFFMANN. New York: Continuum. 1980. Pp. xxiv, 375. \$19.50.

Especially in our present mean era of rattling jingoism, empty bombast, disillusionment, and political blight, this book is an enjoyable change for academics, students, and serious general readers. We have here a broadly balanced interpretive account, by an honest and informed scholar, of relations between the United States and its most important European allies. The original work was published in Germany and France in 1978. There is regrettably no bibliography, but the back notes are useful.

The author, Alfred Grosser, is a French political scientist and multimedia journalist with a progres-

sive bent and a reputation as an expert in West German affairs. He gives his readers a thoughtful, sometimes surprising, view from the other side that is neither animated by rancor nor party. This is a mature and reasonable analysis, one that is both international and European. The following is an example of his attempt to reconcile his attitude with a responsible treatment of his subject: "Every Western European statesman experiences two contradictory feelings: satisfaction that the United States exists, and annoyance that the life and the death of his fellow citizens depend in large measure on the decisions of a foreign and distant President" (p. 323).

This book is divided into four parts and each is arranged in a comfortably chronological fashion with emphases on the more dramatic periods such as 1947 and 1973; each part deals with a decade. The consensus areas, such as the Berlin crises and the evolution of the European Economic Community, are there, as are some fresh sections in part 3 on "The Transatlantic Economy and Vietnam" and "Student Unrest and Monetary Turmoil."

In Grosser's view, "Western Europe does not constitute a unity" (p. xiii). He has therefore mainly devoted his efforts toward trying to disentangle what, for too many Americans, has been and continues to be an illusory front: a group of nations that supposedly stands in opposition to American trade, monetary, political, and defense policies. The author's refutation is, of course, correct; but it takes a kind of perseverance, study, political savvy, familiarity, independence, and discrimination that most of us—and more disturbingly the politicians and common information sources we rely on—are unwilling to exercise. This book is a convenient remedy of the sort we should use if we are to cure our current intellectual sloth and backsliding in the international arena.

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ANCIENT

BORIMIR JORDAN. *Servants of the Gods: A Study in the Religion, History and Literature of Fifth-century Athens*. (Hypomnemata, number 55.) Göttingen: Vandenhoeck and Ruprecht. 1979. Pp. 128.

Borimir Jordan's small book discusses fifth-century attitudes toward the relation of divine and human action by a detailed (and often technical) analysis of certain important texts, in particular the "Hekatompedon Decree" (IG I²4), the Defense of the Acropolis against the Persians in Herodotus, and the double burial of Polyneices in Sophocles' *Antigone*. A long first chapter furnishes a new text and extensive commentary of the decree; its main thesis is

that this text, which was probably inscribed in 485–84 B.C., is a copy of an older set of prohibitions regulating cults on the Acropolis, of about the middle of the sixth century. There are valuable comments on the archaic terminology embodied in the decree, but this reviewer is not convinced that all the regulations are equally old—witness the difference between large and small fines and differences in vocabulary within the decree itself. The new text is not based on autopsy, and new editions are expected in the forthcoming IG I³ and by F. Preisshofen (*Arch. Anzeiger* [1977]: 75–80).

The second chapter is devoted to the functions of temple officials, especially the treasurers whose duties were much broader than the name implies; they were the successors of the famous *naukraroí*. Chapter 3 turns to Herodotus's account of the Persian siege of the Acropolis, which was defended by the treasurers and certain poor men. Jordan concludes that the former were fulfilling their religious duty (compare the prophet at Delphi during the Persian attack on the sanctuary) and only the poor were misled by the wrong interpretation of the Wooden Wall Oracle. Throughout, Jordan exhibits an almost unbounded confidence in the accuracy of Herodotus—a confusion, I think, of using him as a source for religious and intellectual attitudes of the period with excessive trust in the accuracy of the traditions he transmits. The Wooden Wall Oracle is considered genuine; characteristically there is no discussion of the mention of Salamis in it.

Chapters 4 and 5 are important notes, the first stressing that the mention of priestesses on the Acropolis argues against the authenticity of the Themistocles Decree, the second contending that the state's treasure was evacuated to Salamis and thus survived the war. Chapter 6 attempts an interpretation of Sophocles' *Antigone*, especially of the central character, Kreon, who is considered a typical representative of the Athenian view of the relation of gods and human politics. When he turns against the gods in the course of the play, he is twice warned by the gods themselves burying Polyneices (the idea that the two burials—a notorious crux—are both carried out by the gods is a development of the thesis of M. McCall [*JCS*, 22 (1972): 103–17] that the first burial was made by the gods). Little is said about *Antigone* in this chapter. An appendix discusses fires, sacred and profane, on the Acropolis.

The book is well written (all Greek passages appear in English), and the focus on the reality of religious sentiment in the early fifth century down to the *Antigone* makes for stimulating reading. It is in the nature of the evidence and of the amount of past scholarship on this period that much of the argument depends on precise interpretation of specific passages. Here there is much scope for disagreement, but the solutions proposed are always

interesting. Readers not well acquainted with the scholarship of the fifth century should be aware that in many cases judgment must be reserved to specialists.

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CHESTER G. STARR. *The Beginnings of Imperial Rome: Rome in the Mid-Republic*. Ann Arbor: University of Michigan Press. 1980. Pp. 76. \$10.00.

The conventional view of Rome in the mid-Republic (338–264 B.C.) is that it was a simple agricultural community, relatively self-sufficient, and obsessed with national security. Chester G. Starr holds that this view neglects or misinterprets contrary evidence and is inconsistent with Rome's steadily increasing domination of Italy during that period, with a government that became ever more complex and responsive to a widening range of the citizens' interests and power.

Starr uses the evidence of the girth of the "Servian" wall to estimate a population in the city in the tens of thousands (30,000 to 90,000), and points out that the well-documented need for an increasing water supply in the period must have been paralleled by a need for the import of grain. This trade was in private hands: whence, then, did the Romans get the money to pay? As evidence for wide mercantile interests, he cites the Roman treaties with Carthage, the presence of foreigners in Rome, the mentions in non-Latin sources of Roman ships overseas, and archeological evidence of a wide distribution abroad of plates manufactured in Rome.

Rome appears to have become affluent during this period, if one can judge from reports of booty, taxes, an extensive building program, and the development of coinage. There also was a growth of private wealth and of extensive contacts with the Greek East. Colonies established on islands off the coast must have had ships, which would control coastal shipping; and Roman naval activities are indicated by the establishment in 311 B.C. of the *duoviri navales*, a treaty with Rhodes in 306, and the report of embassies to and from Egypt; and the war with Tarentum was brought about by the activities of Roman ships off that city in 280. Further, commercial interests alone give a reasonable explanation for Rome's interference in Sicily and the outbreak of the First Punic War.

This is the gist of Starr's evidence and argument. Much is speculative, hinging on the problem as to what the Roman population did for a living before the advent of government subsidy. The notorious sale of political support by the poor to the rich is hardly enough. But years after this period as

chauvinistic a Roman as the elder Cato sent his readers to southern Italy for heavy equipment, while Rome was mentioned as the source only for wearing apparel and light equipment, both normally the products of household industry. This counsels caution in estimating Rome's productivity.

FRANK C. BOURNE
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NOBERT ROULAND. *Pouvoir politique et dépendance personnelle dans l'Antiquité romaine: Genèse et rôles des rapports de clientèle*. (Collection Latomus, number 166.) Brussels: Latomus Revue d'Études Latines. 1979. Pp. 658. 2,500 F.

It was Arnaldo Momigliano who, in his review of Ronald Syme's *Roman Revolution* forty years ago, put his finger on an anomaly that remains essentially unexplained. "The clientship of the old days had been the strength of the senatorial aristocracy. The clientship of the last century B.C. destroyed the senatorial government. Why?" (*Journal of Roman Studies*, 30 [1940]: 78). Momigliano offers no solution to this very central dilemma, any more than Syme had done in his *Roman Revolution*. But Momigliano's question points up the importance of *clientela* to any study of Roman history, an importance that it has not generally been accorded in practice. The *Oxford Classical Dictionary*, for example, devotes only half a column to the subject. The much more lavish scale of Pauly-Wissowa's *Realencyclopädie* enabled von Premerstein to spend fully 32 columns on it, a short article by the standards of that publication, and one largely given over to the legal aspects of the subject.

It is only with Nobert Rouland's study that *clientela* has come into its own at last. The study covers virtually the whole span of Roman history, beginning in the regal period and going through to the end of the western empire a thousand years later. Rouland sees the *clientela* system as a deliberate creation on the part of the patrician aristocracy designed to perpetuate their own political, social, and economic predominance. Though the composition of the Roman ruling elite changed during the Republic, *clientela* survived as a bastion against democracy, only to wither away once monarchy supplanted aristocratic government in the Principate. Rouland refuses to recognize the relationship between plebs and princes as one of clientship at all.

In this respect Rouland's study shows signs of narrowness. Clientship has lately become an important subject in the social sciences at large, and its presence has been detected in societies of every possible type and variety. That *clientela* changed in the late Roman Republic is undeniable, and the reasons for this change will undoubtedly go a long way toward explaining the fall of the whole republican

edifice. By denying the "client armies" of the late republican period the title of clientship, Rouland is scoring a pedantic point at the expense of an understanding of the essential processes at work. In so doing Rouland denies himself a unique opportunity of tackling the central problem isolated by Arnaldo Momigliano in his review of Syme's *Roman Revolution*.

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MEDIEVAL

LUITPOLD WALLACH. *Diplomatic Studies in Latin and Greek Documents from the Carolingian Age*. Ithaca, N.Y.: Cornell University Press. 1977. Pp. xii, 396. \$35.00.

To do justice to this disjointed, obscurely written collection of studies in a few hundred words is impossible. But perhaps it is charitable not to give it its full due, especially for the sake of the author. For it is difficult to believe that this book will add to Luitpold Wallach's stature as a scholar.

For one thing, more than half of the book (parts 1, 2, and 4, comprising 9 of the 16 studies and 215 of the 352 pages of text) is taken up by previously published studies devoted to important Carolingian texts: the *Synodica* of Pope Hadrian II, the *Libri Carolini*, the alleged oath of Pope Leo III, and the documents reporting the Roman synod of December 800. Their republication will do little more than remind Carolingians of Wallach's special talents: his awesome knowledge of late classical and early medieval texts and his adeptness in following the transmission of manuscripts. Those talents are best demonstrated in Wallach's already known efforts to identify the sources that undergird the *Libri Carolini* (*LC*) that comprise most of book 2, announced as a "Prolegomena to a Critical Edition of the *Libri Carolini*" (that edition, according to the book's bibliography, is being "prepared for future publication"). We must await that edition to render a final judgment on Wallach's preliminary investigations.

What then must count is the filling Wallach chose to put between his slabs of wholesome but ordinary "bread," that is, the value of the newly published studies constituting part 3, chapters 8 through 14. The theme of these studies is to demonstrate that the alleged authorship of the *LC* by Theodulph of Orléans is a fiction. The results of Wallach's investigations into the important problem of the authorship of the *LC* are disappointing, chiefly because he approaches this issue in a singularly unproductive fashion. Wallach seems to have only one real concern: to discredit everything that Ann Freeman has said in presenting her persuasive case supporting Theodulph's authorship of the *LC* (see *Speculum*, 32 [1957]: 663-705; 40 [1965]: 203-89; 46

[1971]: 597–612; on page 371 Wallach's page citation for the first of these articles is in error). Since Wallach does not see fit to recognize Freeman among those who have done "basic studies" dealing with the *LC* (see p. 43 n. 1), his animus toward her may mystify most readers. (Incidentally, she is preparing a new edition of the *LC* for *MGH*; see *Deutsches Archiv*, 31 [1975].)

All will quickly realize that Wallach's obsession distorts his scholarship. His basic tactic is to evoke a particular patristic or biblical source that *might* be an alternative for every text that Freeman used to support Theodulph's authorship of the *LC*. The discovery of any possible *other* source upon which the author of the *LC* might have depended settles the case: Freeman is wrong and Theodulph is *not* the author of the *LC*. This approach, tediously pursued without any apparent logic except to convict Freeman, is abetted by frequent misrepresentations of her position, a transgression of the code of scholarly fairness for which Wallach will be exposed. On occasion Wallach is led to ridiculous positions, as, for example, in his response to the suggestion of stylistic similarities between the *LC* and Theodulph's other writings or in his treatment of the possible connection between the *LC* and the iconographic themes represented in the oratorium built by Theodulph at Germigny-des-Près, that make one wonder whether Wallach understands what the *LC* said concerning the use of images.

In the end Wallach does not make a convincing case disproving Theodulph's authorship of the *LC*. Nor will he find many ready to accept his confident statement that his investigations have led him to "believe that Alcuin's authorship of the *Libri Carolini* cannot be questioned" (p. 289). Even a successful demonstration that Theodulph did *not* write the *LC* would not suffice to prove that Alcuin did. Wallach will have to substantiate the assertions he offers in his brief chapter 14 as proof of Alcuin's authorship of the *LC* before he can claim to have contributed to the resolution of an important historical problem. One wishes that those who decided to publish this book had asked this of him in order to justify its publication in the eyes of those scholars who seek desperately to find outlets for studies that are both original and previously unpublished.

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FRANCIS OAKLEY. *The Western Church in the Later Middle Ages*. Ithaca, N.Y.: Cornell University Press. 1979. Pp. 345. \$17.50.

This is a welcome contribution to the arduous task of rescuing the late medieval church from that almost uniformly dismal picture painted by high

medievalists, Reformation historians, and even many late medievalists. All three groups of historians can read this book with hope of reward, for Francis Oakley not only ranges widely but also stresses again and again the continuities between the late medieval church and both the earlier and the later periods. In successive chapters on ecclesiastical government, modes of piety, currents of thought, heresy, reform, and varieties of spirituality (as reflected in six different lives), he offers us the benefits of years of reading and reflection. The tone throughout is almost disconcertingly calm and balanced; this, plus Oakley's emphasis on continuity, should prove refreshingly annoying to American Reformation historians, even though some recent German Reformation scholarship has also explored links with the late Middle Ages.

It should be noted that this book is too brief, too selective, and sometimes too advanced to constitute a handbook or a survey. One can scarcely do full justice to such a complex subject in 300 pages that, furthermore, vary somewhat in level and quality of treatment. As one might expect, Oakley's forte lies in *Geistesgeschichte*, and so, for example, the pages on Wyclif, Hus, and conciliarism are particularly worthwhile. Yet some readers will surely find unsatisfying the treatment of the crucial distinction between the *potentia absoluta* and the *potentia ordinata* of God, which Oakley does not explain altogether lucidly and which he effectively presents as the linchpin of both late medieval mystical and Scholastic thought. And if one were not already acquainted with the relevant literature, one might easily obtain the entirely erroneous impression that the Scholastics were preoccupied only with the divine *potentia* and made no contributions to economic theory, political thought, science, epistemology, law, and logic. Specialists in other areas will doubtless raise other objections, no matter how much they will have learned from this book in other respects.

Nor does Oakley draw as clearly as he might the possible conclusions from his discussion or delineate as unambiguously as he ought the points of continuity between the late Middle Ages and the preceding and especially the later periods. Thus, in his otherwise very good synthesis of the problems and the position of the papacy he does not, I think, sufficiently stress that thoroughgoing reform of the existing church required the determination and the cooperation of both ecclesiastical and secular authorities and that neither could go it alone without the other. Similarly, although high medievalists should be able to infer from the text many underlying continuities over a half millennium of church history, Reformation historians will be much less happy, partly because here the thesis of continuity is itself more problematical, partly because in any case Oakley does not develop this side of his thesis

systematically and lucidly enough. What should stand as cogent arguments are often left as elusive suggestions. Finally, readers in all camps will be disturbed by numerous errors, some presumably typographical in origin (for example, "moral sin" on page 308), others clearly authorial (for instance, 1269 instead of 1265 on page 49; the beginning of the Black Death in 1347 on page 115; differing birthdates given for William of Ockham; and so forth). Nevertheless, despite these failings, small and otherwise, every reader can profit in knowledge and in understanding from this stimulating, thoughtful, yet incomplete book.

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CAROLINE WALKER BYNUM. *Docere Verbo et Exemplo: An Aspect of Twelfth-Century Spirituality*. (Harvard Theological Studies, number 31.) Missoula, Mont.: Scholars Press. 1979. Pp. xvii, 226. Cloth \$15.00, paper \$10.50.

In this scholarly monograph, Caroline Walker Bynum has tackled a subject of difficult and clear importance, twelfth-century spirituality. Her basic thesis is that behind the constellation of forces at play in the post-Gregorian reform movement, a new focus began to emerge. This new focus of the Christian life was "to serve one's fellowmen, through charitable acts and preaching, to the duty to love and worship of God . . . to live not merely without private property but also in corporate austerity" (p. 2).

Although this is not a new thesis, it is the theme of Bynum's book, and her approach to the topic is fresh and appealing. By utilizing the "in house" treatises written by monks and regular canons for themselves and to each other, she has turned to a body of literature little studied by scholars. Through meticulous historical investigation, the author has made a careful study of treatises of practical advice concerning the cloistered life, expositions on rules, and commentaries directed to the novitiate. By sharply focusing on her detailed and carefully documented analysis of the topic, Bynum concludes that monks and canons saw themselves as teachers to each other, *verbo et exemplo*.

The monograph is divided into two parts in order to explore individual concepts of religious life first among the regular canons and then among the brotherhood of monks. In each part, the author presents an in-depth analysis of the sources followed by excerpts from individual treatises. At the conclusion of each section, the author explores the meaning of example and edification through the insight she has gained from pursuing the subject in total context.

To the historian of monastic spirituality, Bynum's monograph raises a number of special problems. For example, one is left with the feeling that the increased interest and feeling of responsibility for one's neighbor, the new concern for incorporating an awareness of duty toward one's fellow man to teach by preaching and by example, never quite got beyond the talking stage during the twelfth century. Bynum concludes that the older monastic tradition, a mental conservatism, was too strong for this new Christianity to grow beyond the minds of individuals or, at best, beyond the walls of a single monastery.

The problem with a monograph of this kind is that an author's vision must be narrow. Thus, the tendency to esteem the materials and the authors who produced them is hardly avoidable. Often previous events and later events are ignored because of the monograph's single-minded purpose. Most obviously, "docere verbo et exemplo" became the motto for new religious orders, such as the Franciscan, in the thirteenth and fourteenth centuries. Yet, Bynum hardly speculates on the influence of her selected writers on subsequent developments. One hopes another monograph will continue this aspect of spirituality into the later Middle Ages.

The specialist will want to examine Bynum's arguments in more detail while the generalist will find much useful information affecting a wide variety of religious history. The study clearly adds to our knowledge of the twelfth century and to twelfth-century spirituality. The book is well documented; it contains a superb bibliography and a useful index.

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FÉLIX GRAT *et al.*, editors. *Recueil des actes de Louis II le Bègue, Louis III et Carloman II, rois de France (877-884)*. (Chartes et Diplômes Relatifs à l'Histoire de France.) Paris: Imprimerie Nationale. 1978. Pp. cxvii, 316.

JEAN DUFOUR, editor. *Recueil des actes de Robert I^{er} et de Raoul, rois de France (922-936)*. Under the supervision of ROBERT-HENRI BAUTIER (Chartes et Diplômes Relatifs à l'Histoire de France.) Paris: Imprimerie Nationale. 1978. Pp. cxxii, 273.

These two volumes complete the critical editing of the earliest royal diplomas of France (840-987), a project of the Académie des Inscriptions et Belles-Lettres now almost one hundred years old. The set is fundamental to research on late Carolingian (pre-Capetian) west Frankland: it comprises three volumes on Charles the Bald (840-77), edited by Georges Tessier (1943-55); one volume on Louis II, Louis III, and Carloman II (877-84), edited by Fé-

lix Grat *et al.* (1978); one volume on Eudes (888–98), edited by Robert-Henri Bautier (1967); two volumes on Charles the Simple (893–923), edited by Philippe Lauer (1940–49); one volume on Robert I and Raoul of Burgundy (922–36), edited by Bautier and Jean Dufour; one volume on Louis IV (936–54), edited by Lauer (1914); and one volume on Lothair and Louis V (954–87), edited by Louis Halphen (1908). Only the latter two volumes antedate the Second World War, but the basic research for almost all of them had been done as early as 1900–06. It was Tessier who renewed the early initiatives, establishing in his *Charles le Chauve* the basic principles of late Carolingian diplomatics that eased the tasks of the subsequent editors; and in recent years Bautier has supervised the completion of the last phase of the Académie's enterprise. No fewer than three scholars had tried to edit the acts of Louis the Stammerer and his successors before Bautier pulled their dossiers together to finish the work. In the case of Robert and Raoul, he was more fortunate, one gathers, in that less had been done to confuse matters when Jean Dufour undertook the edition.

The two new volumes correspond to critical phases in late Frankish political history. It was precisely the inability of the successors of Charles the Bald (including Charles the Fat) to mobilize effectively against the Northmen that led to the unprecedented election of a non-Carolingian king in 888. A Carolingian restoration followed; but Charles the Simple was deposed in 922, the victim of western magnates with whose loyalty he had trifled, and was succeeded, years before his own death, by the magnates whose acts Dufour and Bautier have edited. Robert, the brother of the former king Eudes and the grandfather of Hugh Capet, died in battle against Charles the Simple in 923; his son-in-law Raoul of Burgundy (923–36) had to struggle for years to retain the crown. But their diplomas tell us little about the troubles of these embattled kings—at least, little directly. The numbers of extant diplomas decline from an average of eleven per year in the later ninth century to two in the time of Raoul, a fact consistent with Jean-François Lemarignier's inference from less complete evidence that the king's privilege was losing its value in the tenth century. The diploma was politically useful to its bestower, but only when it was wanted. In a valuable excursus, Dufour employs the diplomas together with scattered local evidence to plot the spreading influence of Raoul of Burgundy.

The editors of both collections pay scrupulous attention to the forms and procedures of the chancery. Although most of the diplomas of Louis II, Louis III, and Carloman II were drafted by royal clerks, nearly half of those of Robert and Raoul were the work of their beneficiaries. A privilege

written at Saint-Symphorien of Autun in 925 is shown to be substantially a "private" charter in public (that is, royal) form. An increasing irregularity in the formulas likewise suggests to Dufour "the decadence of the royal chancery." One change overlooked by Tessier is the telescoping of *Actum* and *Datum* into a single clause governed by *Actum*; but this can hardly have been adopted from Provençal diplomatics in the early tenth century, as Dufour speculates, for it appears already with striking regularity in the early diplomas of Carloman II (nos. 50–53[881]).

The texts of both collections are presented meticulously and consistently according to the *chartiste* norms now standard in the series *Chartes et Diplômes*; the indexing and printing are sumptuous.

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KARL BRUNNER. *Oppositionelle Gruppen im Karolingerreich*. (Veröffentlichungen des Instituts für Österreichische Geschichtsforschung, number 25.) Vienna: Hermann Böhlau Nachf. 1979. Pp. 224. DM 80.

This is the most important book on Carolingian history published during the last three decades. Louis Halphen's 1947 description of the Carolingian Empire was monolithic—as if the written instructions of the Capitularies were a description of what actually had happened. (F. L. Ganshof, in a large number of minute studies, shed light on the tensions, irregularities, and imperfections of the Empire, but he never shaped his unsurpassed erudition into a comprehensive book.) Then came Heinrich Fichtenau's book, first published in German in 1949 and translated into English in 1957, which concentrated systematically on the tensions and improvisations, dubious loyalties, and imperfect learning of the Carolingians. Fichtenau's realism was based on reading between the lines and on the reasonable assumption that the nearly streamlined consensus underlying Halphen's story must be deceptive. Since then we have had more detailed studies by Ganshof, but no synthesis. I have tried to probe the political tensions behind the events of 799–800 in my *Origin of the Carolingian Empire* (1960).

Now, at long last, Karl Brunner has made an attempt to base such probings on concrete evidence. Minutely combing all the well-known sources, he has managed to provide lists and biographies of a large number of individuals and families whose support, tacitly taken for granted by Halphen and reasonably doubted but rarely specified by Fichtenau, for Charlemagne's grand schemes was lukewarm and who often opposed them. In this way, he

has made an enormous contribution to our understanding of the political realities and social tensions that prevailed in the Carolingian Empire. English-speaking readers will recognize at once that we have here something like an application of Louis Namier's method to Carolingian history.

All the same, the limitations of the sources, which not even the best of historians can transcend, impose obvious limits on the present book. For the reign of Charlemagne itself we are provided in the first chapter with nothing more than different legal terms for "opposition" and in the second chapter with sketchy biographies of prominent magnates—unfortunately and unjustifiably described as "nobles" (*Adel*)—with the longest section devoted to Tassilo, the man we already know most about because the sources are most plentiful. In any case, Tassilo of Bavaria was marginal to the Frankish Empire as such, and his enmity is not comparable to the opposition of, for example, Charlemagne's son, Pepin. Chapters 3 through 5 concentrate on the part of the story that is already well known, that is, the difficulties encountered by the reforms of Louis the Pious and the quarrels among his sons and successors.

Such lack of balance, however, is necessitated by the sources, and it cannot detract from the enormous step forward. It may now be possible to assemble Ganshof's research and feed it, together with the detailed information secured by Brunner, into the Fichtenau synthesis and combine them all together with a theory about power, feudalism, and monarchy to provide a genuine sociopolitical history of the ninth century.

PETER MUNZ
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GÉRARD SIVERY. *Structures agraires et vie rurale dans le Hainaut à la fin du moyen âge*. Volume 2. Lille: Presses Universitaires de Lille. Pp. 364–729.

The first volume of this study dealt with social groups and the rural economy through the early fourteenth century and with field patterns through the early sixteenth. The appearance of the second volume, continuing the discussion of economic and social developments into the sixteenth century, largely obviates the problems noted in the first.

Gérard Sivery considers Hainaut as part of a vast economic region of northwestern Europe dominated by the markets of the great cities. Grain prices form the basis of most of his general conclusions. In contrast to the situation in England and much of France, the decline in grain prices after the famine of 1315 was not permanent in Hainaut, and prices rose again between 1334 and the mid-1370s. They then declined until the end of the century, fol-

lowing the general European pattern, and rose again slightly until 1437. Despite the price rise, grain producers lost half their purchasing power between 1315 and 1534. Hence many converted substantial amounts of arable land into pasture. The early fifteenth century saw a gradual rise of grain prices, with a brief sharp rise after 1437, but there were tremendous fluctuations for the rest of the century, with the curve generally descending except between 1470 and the 1490s. Land prices and rents rose sharply, but wages remained stable, suggesting distress in the countryside.

Sivery's remarks on rural society are less original and controversial than those on the economy. "Class" barriers had never been severe in Hainaut. Nobility was not based on fixed rules of descent but on a combination of knighthood, wealth, possession of a seigneurie, and recognition of noble standing by the community at large. Sivery devotes surprisingly little attention to the peasantry. Population declined sharply in the late fourteenth century. There was some recovery between 1400 and 1431, but then a severe drop lasting into the early sixteenth century began, particularly in the war-plagued south. Evidence from censuses suggests considerable instability in peasant society. Sivery's understated conclusion is that the great lords, not the small farmers, were the primary beneficiaries of the conversion to pasture, and he describes quarrels of village organizations against lords who enclosed commons.

This is a fundamental and solidly based study, but its methodology and conclusions are open to question. Sivery realizes that extremely high grain prices are not necessarily a sign of prosperity; indeed, his figures show them reflecting famine, plague, and wartime devastation and presaging a long period of gradual price decline. Despite a severe population drop, Sivery insists that this is not the classic crisis of overproduction for a reduced market. He argues, furthermore, that grain prices within Hainaut were largely determined by the export market in the Flemish cities, although he admits that this can have absorbed no more than 6 to 10 percent of the total production of Hainaut even in peak years. Properly concerned with placing Hainaut into a larger regional context, Sivery neglects the effects of the urban markets within Hainaut and in areas slightly west of Hainaut on developments in the rural economy of the principality. One would also have welcomed more detailed work on village populations and social structures. Sivery has written a valuable study, but much remains to be done, particularly on the peasantry.

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K. J. LEYSER. *Rule and Conflict in an Early Medieval Society: Ottonian Saxony*. Bloomington: Indiana University Press. 1979. Pp. x, 190. \$25.00.

K. J. Leyser's volume on Ottonian kingship and its Saxon origins is a welcome addition to tenth-century-oriented scholarship. Rather than look at these issues from a political or constitutional viewpoint, the author has chosen to use certain anthropological insights in order to examine the conflicts of the Saxon nobility with their kings.

The book contains three discussions. Leyser begins by focusing on Otto I's early reign and the frequent rebellions against his government, first instigated by his brother Henry, and later coalescing around his son Liudolf and son-in-law Conrad the Red. One interesting observation points to the fact that Saxon nobles rarely acted on their own initiatives against the king but tended to attach themselves to cadet members of the royal house whose discontent they encouraged and whose rebellious activities they exploited for their own benefit.

Leyser continues with two chapters on Saxon aristocratic women, mainly about their ability to inherit vast portions of the family wealth. Rich widows and unmarried noblewomen often used their lands to found convents, thereby protecting their own persons and their wealth. A royal charter gained them additional independence from their male kin, who undoubtedly disliked this diminution of their lands but who could not force the women into marriage as long as royal protection remained strong.

Finally, Leyser turns to a discussion of sacral kingship, the famous *rex et sacerdos* concept, the expression of the priestly functions of the king and his "separateness" from other members of the ruling classes. From an introduction into the meaning of such sacral kingship, the author turns to its social manifestations: the ritual crown wearing at great feast days and the all-important royal progress through the kingdom. He also discusses the reaction of the lay nobility to this cult of the king and introduces some notion of the wide gap between the ideals of priestly writers and the realities of daily life, as lived both by the sacral king and the nobles. Further, the author seeks to examine the frustrations of the Saxon aristocracy at not becoming a favored "imperial" nobility as the Franks had been under Charlemagne.

In his introduction, Leyser stresses the tenth century and its strangeness, commenting about "the heroic mould of its values," and the "unashamed ferocity of its conflicts" (p. 1). One might add that in any century the beliefs and norms tend to contrast violently with the realities of the political and social arena, no less so in the twelfth century than in the tenth. What perhaps contributes to the author's

feeling of strangeness is his use of the characters of his studies as symbols rather than as living beings, one unfortunate by-product of analyzing historical material in an essentially unhistoric way. It is quite beneficial to talk about Otto I as representing a sacral king who exhibits both terror and magnanimity to impress his contemporaries, thereby serving as a model of sacral kingship per se; on the other hand, Otto I was also a positive, strong-willed, and energetic ruler who was determined to bring order to a brutal environment and who, undoubtedly, could fight fire with fire. Thus his reaction to clannish Saxon greed and their unwillingness to see the larger issues of the Reich had sound political reasons not necessarily based on his offended sacrosanctity.

All this does not deny the obvious value of Leyser's studies or of the detailed factual information he has gleaned from the pages of Thietmar of Merseburg and other early historians; rather it points to the limitation of his approach to the subject matter. It is no longer fashionable nor, perhaps, desirable to write purely political history, but we might say that in our attempts to examine criteria from new angles, we forget that the unique human qualities of historical personages are equally essential to our understanding of an age. In using people as points of reference to an investigation of social norms and structures, these human qualities are often lost.

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HAGEN KELLER. *Adelsherrschaft und städtische Gesellschaft in Oberitalien, 9. bis 12. Jahrhundert*. (Bibliothek des Deutschen Historischen Instituts in Rom, number 52.) Tübingen: Max Niemeyer Verlag. 1979. Pp. xiii, 463. DM 108.

This substantial monograph is a thorough revision, with many additions, of the author's *Habilitation* (1971) and is intended to clarify and supplement various aspects of north Italian socioinstitutional history within the compass of the kind of work carried out by Violante and his students. Hagen Keller's study is divided into six chapters not including a useful introduction, a 20-page conclusion, two appendixes dealing with prosopography, a good 30-page bibliography, and four indexes.

Of the six chapters that form the core of this study, the first five deal essentially with the dynamics and structure of socioconstitutional developments within the north Italian region, and, as might be expected, Milan and its environs receive the greatest amount of attention. Keller works backward from the twelfth century to argue for the continuity of the noble class that he sees originating

from a union of Frankish administrators and Lombard *Grundherren*. The broad scenario that he presents is one with which specialists in the history of post-Carolingian *Francia Occidentalis* will not be unfamiliar. In the process of creating a noble class, the protagonists who had had some early connection with royal and ecclesiastical power gained in importance because the monarchs and the church sought to use them as a counterweight to the power of the *Reichsaristokratie*. These *Grundherren* built military strongholds (often with royal support), obtained the power of the ban, and gained immunity from royal or imperial officials. Through a great variety of means and especially through the church, these men became prominent in the towns and an urban-rural nobility was formed. Union of the *capitanei* and the *valvassores* provided an important later step in the formation of this nobility, which Keller contrasts with both the *rustici* of the countryside and the *populares* in the city, that is, the *Volk*. Thus Keller maintains: "Eine Gegenüberstellung von 'Adel' und 'Bürgertum' ist in Italien nicht möglich, sondern allenfalls die von 'Adel' und 'Volk' innerhalb der 'Bürgerschaft.'"

Unfortunately, in a review of this length it is not possible to examine the vast array of particulars developed and clarified by Keller. It is vital, however, to emphasize that the great strength of the continuity argument rests upon prosopographical reconstruction that, as far as it goes, is compelling in individual cases. In the nature of this method there is, however, a prevailing bias in favor of continuity because even when there is no evidence for continuity in a particular family we cannot be sure that the family is a new one. It may be an old and established family whose traces have been distorted or obliterated by the vagaries of time. Further, families without an honorable past have a well-known tendency to create one from the whole cloth. Thus, I am not totally convinced by Keller's conclusion that in the aggregate he has found a surprising degree of continuity.

Keller's sixth chapter is, in part, devoted to a comparison of "feudalism" in northern Italy with conditions in other regions of the post-Carolingian world. Scholars who have a penchant for working with constructs such as "feudalism" or, indeed, for a "Carolingian Vassalic Pyramid" will find Keller's observations of interest. This reviewer, however, is unsympathetic to such exercises. These quibbles must not mislead the reader. Keller has written an important book that is a valuable supplement to the work of Violante and his school. It must be read and, indeed, studied by anyone interested in the *potentiores* of the early medieval West.

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JOSIP LUČIĆ. *Obrti i usluge u Dubrovniku do početka XIV stoljeća* [Crafts and Services in Dubrovnik to the Beginning of the Fourteenth Century]. Summary in Italian. (Monografije, number 7.) Zagreb: Sveučilište u Zagrebu—Institut za Hrvatsku Povijest. 1979. Pp. 284.

Josip Lučić's *Crafts and Services in Dubrovnik to the Beginning of the Fourteenth Century* analyzes an important aspect of life in the city-state of Dubrovnik. The greatest portion of the book focuses on the last two decades of the thirteenth century, since the archival material in that city starts to be systematic in the year 1278.

In the first part of the volume, Lučić surveys the crafts of builders, woodworkers, metalworkers, goldsmiths, textile workers, tanners, pharmacists, and those engaged in food supply and preparation. Then follows a survey of "services" that includes intellectual occupations: doctors, painters, and teachers as well as various other professions. Attention is paid to slaves, various groups of servants, and even mistresses. Each category is introduced by a general explanation (sometimes too elementary) of the particular craft or service, followed by lists of craftsmen, professionals, slaves, servants, mistresses, and so on, with all available data on each one of them.

The second part of the book is meant to illustrate the position and significance of crafts and craftsmen in Dubrovnik in the thirteenth century. It details the numerical situation of various crafts and the qualifications and origins of the craftsmen. Male and female names are divided into Slavic and non-Slavic groups. This is followed by descriptions of craftsmen's workshops and their tools, processes of production and marketing, and business associations and credit operations of the craftsmen. Lučić discusses the role of craftsmen as landowners and homeowners in Dubrovnik and its territory, the attitude of craftsmen toward dowries, the social position of craftsmen, their fraternities, and their relationship to the city authorities. The education and origins of apprentices are surveyed, as is the situation of slaves and servants.

After a brief conclusion, the third section of the volume presents 25 pages of original archival documents relevant to the subject, a summary in Italian, and three indexes, which are not always as accurate as one would wish.

As Lučić himself states, his book is important "as documentation, materials" (p. 22). Indeed, there is no denying the great value of the archival sources on which it is based. One might regret, however, the lack of a broader, comparative approach to the subject. The economic, social, and political impact of craftsmen in Dubrovnik could have been compared to that of merchants and patricians, the relations between those groups could have been exam-

ined. Comparisons with Venice, Florence, and other cities would have been most interesting. Lučić is a well-known expert in the archives of Dubrovnik, and such discussion on his part would have been worthwhile. Nevertheless, his detailed presentation of a large amount of archival data gives enduring value to this volume.

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MODERN EUROPE

BRUCE MANSFIELD. *Phoenix of His Age: Interpretations of Erasmus, c 1550–1750*. (Erasmus Studies, number 4.) Buffalo, N.Y.: University of Toronto Press. 1979. Pp. xviii, 348. \$25.00.

Bruce Mansfield's interesting study charts a course through the many and varied interpretations of Erasmus as a religious intellectual from his death in Basel in 1535 up to the first full-scale biographies of the great humanist written in the mid-eighteenth century. The volume does not try to be exhaustive. Instead, selected cases have been chosen to show patterns of interpretation that developed.

Some of the first evaluations of Erasmus by fellow humanists portray him as the greatest Christian humanist who led in advancing humane letters and exhibiting true religious spirit and who opposed the worst of Catholicism and the Reformation. By the middle of the sixteenth century and after the council of Trent, Erasmus came to be re-evaluated along party lines. He was a poor Catholic, a bastard, a lapsed monk who died in a Protestant city without the last rites. His views led to Lutheranism and Arianism. In Catholic Europe, Erasmus was then "seen more and more as a corrosive influence, weakening structures and easing the way for the assault of the heretics" (p. 48). Among Catholics, Erasmus's reputation underwent a decline throughout the sixteenth century.

The Protestant evaluations varied from place to place and from sect to sect. Erasmus was remembered for his crucial role in the early Reformation. He did refuse, however, to break with Rome, and he did oppose Luther on an issue crucial to the Reformation. For radical reformers he made Arianism respectable. Most sides saw him as indecisive and weak in coming to grips with the titanic struggle within Christendom.

Mansfield follows the interpretations, showing that Erasmus began to get a better press in his native land and during the Arminian struggle when liberal Protestants like Grotius saw themselves in the Erasmian tradition.

At the end of the seventeenth century, after being pelted by various kinds of Catholics and Protes-

tants, Erasmus began to emerge as the heroic, enlightened ancestor of modern man. First Pierre Bayle, "le philosophe de Rotterdam," painted Erasmus as a truly illustrious figure who had been much maligned by all sides. Bayle corrected many details from previous accounts and portrayed the humanistic Christian as a kind of human ideal. Bayle was followed by his enemy Jean Le Clerc in renovating Erasmus's reputation. Le Clerc saw Erasmus as a great religious liberal who used reason as his guide. Le Clerc's achievement, publishing the *Opera omnia* of Erasmus, made the master's works available.

The last section of Mansfield's book deals principally with Enlightenment biographies by Samuel Knight, John Jortin, and Jean Levesque de Burigny. In these works Erasmus has arrived at his modern status, the great scholarly, witty, and ironic spiritual forerunner of the Enlightenment.

This study goes through much well-known and obscure material. It presents an interesting picture of the decline, fall, and recovery of Erasmus's reputation and its relationship to factors in the religious history of the time. The ordering of the material sometimes seems a bit arbitrary, shifting from geographical to theological to chronological. Seeing how Erasmus was interpreted from 1550 to 1750 is an interesting measure of the change of religious climate in the two centuries. Whether this throws any light on the larger question—what were Erasmus's actual views—is hard to tell. Mansfield shows a preference for the interpretations from Bayle onward but makes no real effort to justify this preference. He indicates that he intends to carry on his study beyond the Enlightenment, which should be most helpful.

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ANTONY BLACK. *Council and Commune: The Conciliar Movement and the Fifteenth-Century Heritage*. London: Burns and Oates or Patmos Press, Shepherdstown, W.Va. 1979. Pp. 253. \$25.95.

Antony Black has not written a history of the council of Basel but rather an essay on the political theories of a number of its participants. He does not challenge the usual textbook interpretation of Basel as a council that, through its radicalism and its renewal of the threat of papal schism, brought councils into disrepute. But he does provide a moderate context for Basel's radicalism. He notes that it was the "middle-class," "workaday" clergy who dominated the council and that these men were reformers, not revolutionaries. They merely hoped to extend to the universal church the successful governmental patterns of the numerous collegiate

and communal clerical organizations, the universities, and the city-states, which flourished around them.

The path for realization of this hope lay athwart a serious difficulty: how was a small group like a council to explain its authority or manage its relationship to the whole body it governed? Black claims that Nicholas of Cusa unsuccessfully raised the possibility of elections and true representative government. Many conciliarists, however, simply assumed the council to be the assembled church. They provided no role for the laity at Basel and claimed for the council many of the autocratic powers they denied the pope. The council of Basel might, with some justification, be dismissed as a mere squabble among clergy for control of the church.

Black challenges several popular assumptions about conciliarism. He objects to an easy equation of philosophical realism with papal prerogative and nominalism with conciliarism. Many of the arguments coming out of Basel seem to have defended the council against the pope by recourse to realistic propositions. Black objects to the use of Walter Ullmann's distinction between "descending" and "ascending" theories of authority in attempts to explain the thought of men like Juan de Segovia. He also cautions against an automatic assumption that conciliar arguments led directly to secular parliamentary experiments.

Black touches on several ideas that deserve much fuller treatment. His occasional references to relationships between conciliarism and Protestantism leave loose ends. He suggests a direct line of development from the frustration of the conciliar movement at Basel to the Reformation of the sixteenth century (p. 48), but he also notes that the Protestant assertion of the priesthood of all believers would have been no more compatible with Basel's claims to conciliar infallibility than it was with the monarchical papacy. The book concludes with a controversial ten-page postscript that sketches a short history of conciliarism from Basel to Vatican II. In light of recent examples of the exercise of papal authority and the disciplining of its critics, Black's prediction of a revival of an effective, modern conciliarism may be overly optimistic.

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JOHN F. MORLEY. *Vatican Diplomacy and the Jews during the Holocaust, 1939-1943*. New York: KTAV Publishing House. 1980. Pp. xvii, 327. \$20.00.

John F. Morley, a Catholic priest and educator, no doubt undertook this book because of his intense engagement in the ecumenical movement. His pur-

pose is to determine whether the Vatican's wartime diplomatic efforts were directed only toward the church's interests or also toward the humanitarian concern for the persecuted Jews in Europe.

For his primary sources Morley had to rely on the nine published volumes of the Holy See's records and documents for the years 1939-43, estimated to number over four thousand. In addition, he had access to the Myron Taylor archives at the Roosevelt Library at Hyde Park and to Jewish archival collections in Israel, France, and the United States.

Morley chronicles the activities of Vatican diplomats as they affected Jews and Jewish converts to Catholicism in Croatia, France, Germany, Italy, Poland, Rumania, and Slovakia. His focus is narrow, given his self-imposed limitation to deal only with Vatican diplomacy rather than with the complex questions involving Catholic-Jewish relations in each country and the position of the church vis-à-vis each particular government.

After surveying each country, Morley renders an accounting of the Vatican's diplomatic efforts on behalf of the European Jews. He considers to what extent such efforts were made, how forceful they were, how successful, and what more might have been done to have rescued the beleaguered Jews. He concludes that the papal nuncios had not given much evidence of "consistent humanitarian concern about the sufferings of the Jews during the years 1939 to 1943" (p. 196) and that Pius XII failed to use the "full potential" of Vatican diplomacy "when confronted with the Jewish catastrophe" (p. 208). He does not charge the pope with anti-semitism but concludes that the pope pursued a policy of prudence to protect the Vatican's interests and was not motivated by humanitarian concerns, at least with regard to the fate of the Jews.

Morley's book does not add materially to what we already know about Vatican policy toward the persecuted European Jews (see Guenter Lewy, *The Catholic Church and Nazi Germany* [1964] and Saul Friedländer, *Pius XII and the Third Reich* [1966]). What is new and welcome is Morley's forthright and unapologetic approach to his subject matter. Although he remains always respectful of his church, he has not hesitated to draw conclusions about its moral failure in confronting the Third Reich's murder of six million European Jews.

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PAULA SCALINGI. *The European Parliament: The Three-Decade Search for a United Europe*. (Contributions in Political Science, number 37.) Westport, Conn.: Greenwood Press. 1980. Pp. x, 221. \$18.95.

An analyst of West European politics for the federal government, Paula Scalingi has written a useful

and frequently insightful study of the thirty years that eventually led to the direct election of the 410 members of the European Parliament in June 1979. While the focus is a reasonably detailed political narrative encompassing both events and personalities in the establishment of a viable parliamentary institution for the European Community, the framework is much broader. The progress of West European integration—that is, federalism as opposed to a more or less Gaullist style “union of states” (or confederalism)—is seen as a function of periodic external and internal crises. External crises, such as the Korean War, the Suez crisis, and the invasion of Czechoslovakia, have accelerated the process of regional integration, including efforts in support of a European Parliament. Internal crises, dissension among Community members during times of relative security, as with the 1965 French boycott of the Community, have stalled such progress and inhibited further integration. While such a conceptual framework is useful and traditional, it does establish narrow constraints on the interpretation of key developments. The theoretical works of Joseph S. Nye, Ernst B. Haas, Ronald Yalem, Leon Lindberg, and Stuart A. Scheingold, which examine regional community development in terms of neofunctionalism and a variety of process models, would provide the basis for a more revealing analysis of the vicissitudes of West European Community development. Even so, one can hardly argue with the author’s assertion that the European Community, and hence the European Parliament, has fallen victim to its own successes: “reconciliation of formerly hostile nations, economic prosperity, and détente” (p. 153).

The discussion of the European Parliament in the 1960s is meticulous in its attention to detail and adds substantially to an understanding of the meaning of events, especially from 1965 to 1969. The earlier chapters, which describe the role of the Council of Europe (the Consultative Assembly) and the Coal and Steel Community (the Common Assembly), lack the analysis evident once the author reaches the point of discussing the Common Market. The early period, with the formation of political groups in the Common Assembly or with the rivalry between the Consultative Assembly of the Council of Europe and the OEEC, is important in setting precedents for later developments and requires full discussion. Similarly, greater attention should be given to the policies of national governments (beyond that of de Gaulle) to the role of public opinion and to interest groups (labor, industry, consumers, and others) during both the 1950s and the 1960s. In this sense, the strength of this study is at the same time a weakness: a preoccupation with *Community* institutions, the direct elections issue, and a scrupulously detailed account

of events and the role of key personalities during the 1960s. For each of these, the care in research is evident and the analysis judicious and balanced.

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D. M. LOADS. *The Reign of Mary Tudor: Politics, Government, and Religion in England, 1553–1558*. New York: St. Martin’s Press. 1979. Pp. xii, 516. \$27.50.

D. M. Loads has written a compassionate and scholarly history of a monarch who bears the epithet of bloody and of a reign that has traditionally been denigrated as belonging to the “little Tudors”—short in duration and short on lasting consequences. He has conjoined with his own extensive knowledge and thoughtful observations the research of a host of other scholars to produce a fresh look at a difficult and once-neglected reign. Strangely enough, even when the new research, the new material, and the new approach are placed on record, the conclusions seem to be much the same: Mary, if no longer bloody, is still an “ill-starred” ruler, and her reign, if not portrayed as inconsequential, is still described in oddly convoluted negatives: “By comparison with the great efforts which had been made in the last year of King Edward, both to reduce the debt and to clear up the long backlog of speculation and inefficiency, the activity of Mary’s council in the first year of her reign was not particularly impressive” (p. 187); “She was not an easy ruler to advise. Unlike Elizabeth, Mary was not much interested in finance” (p. 186); “It is not sufficient to label the legislation of these parliaments as conservative and unimaginative” (p. 275); and, “In spite of her reputation for clemency in secular matters, Mary was not noticeably reluctant to execute traitors” (p. 282).

Although the verdict of “sterile” pronounced by A. F. Pollard eighty years ago remains substantially unchanged, Loads has added a dimension of pathos to that harsh dictum—there was, in his estimation, nothing inevitable about Mary’s failure. She came to the throne at a time when the Protestant radicals had lost the respect and loyalty of the bulk of the kingdom and were consumed with a sense of guilt and paralyzed by self-doubt. They might have made just as good losers under Mary as Catholics proved to be under Elizabeth. That they triumphed in the midst of adversity was the measure of the queen’s failure. The stars were sadly out of joint and Murphy’s law operated with a vengeance—everything went wrong that could go wrong—but in the end Loads does not allow the queen to avoid the ultimate responsibility: “Mary’s errors of judgment” (p. 472) brought down upon her the disasters of her reign.

Loades admits that his *Mary Tudor* is "an interim report," but not everyone will be satisfied with his emphasis: three pages on Wyatt's rebellion and the better portion of a chapter on Gresham's finances; six out of fourteen chapters on matters of finance and administration; and the dismissal of the decision to burn nearly three hundred Protestants with the flat and epicene statement that "it was not primarily a policy but rather the discharge by the queen and the cardinal of a clearly conceived pastoral duty. As such, it cannot really be discussed politically in terms of success or failure, but must be seen simply as an aspect of the restored Catholic Church, inseparable from the whole process of restoration" (p. 333). Nevertheless, given Loades's laundry-list style of history, his excessive love of inventories and account sheets, this is by far the best overall history of the reign to date.

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KEVIN SHARPE. *Sir Robert Cotton, 1586-1631: History and Politics in Early Modern England*. (Oxford Historical Monographs.) New York: Oxford University Press. 1979. Pp. xiv, 293. \$36.00.

So many scholars have used the manuscripts Sir Robert Cotton collected, so many have paid tribute to his energy and expertise, that the absence of a modern, full-scale biography is astonishing. Kevin Sharpe's study of Cotton's activities as a collector, antiquary, and politician does much to remedy this, without resolving all the ambiguities.

Robert Cotton was born in 1571 and went to school at Westminster under the antiquary, William Camden. Camden's enthusiasm for British antiquity infected his pupil. Cotton not only joined Camden in the Society of Antiquaries but began as well to build up his library, first collecting materials for a projected history of his own county of Huntingdonshire, then expanding the scope to include any medieval English manuscript (and the coins and monuments of Roman and Anglo-Saxon Britain as well), finally adding even the state papers of his own day. Sharpe's description of how Cotton amassed his treasures, how he stored them, and how he made them available to others is fascinating, but it illustrates as well a recurring weakness of the book. Careful use of the correspondence enables Sharpe to show that Cotton was admired greatly for his collection and his expert knowledge of it and often thanked for his generosity in lending scarce tracts or in answering arcane questions. But, despite Sharpe's claims to the contrary, Cotton remained essentially an intellectual satellite of Camden. The older man was able to take a place in that European community of scholars that included Lipsius,

Casaubon, Gruter, and the rest; Cotton, when he knew such men at all, knew them first through Camden and was able to continue the acquaintance on his own account primarily as owner of a large and varied collection of invaluable manuscripts.

Exaggeration of Cotton's intellectual role does not detract from an appreciation of his real importance, and on Cotton's political connections Sharpe tells us much that is new and interesting. In an age that valued precedents very highly, Cotton had a great store of them. He became, for Northampton, under whom he began his political career, and for others, what Sharpe calls an "expert in politics," a man who wrote position papers and notes for speeches, who sifted projects and occasionally promoted them himself. Cotton's role in the creation and sale of baronetcies, worked out by Sharpe, is a good case in point, for such an addition to the ranks of the aristocracy had inevitably to be defended by reference to the past. After Northampton's death, Cotton continued to do similar tasks for Somerset and Arundel, even for Buckingham whom he disliked and distrusted. At the same time, Cotton enjoyed a career in the House of Commons, and again it is a virtue of Sharpe's book to show how important the quiet Cotton was to the activities of the House. In his first few sessions, Cotton established himself as an expert on the history of procedure; thereafter, despite the lack of continuity in his membership, the Commons regularly turned to him for advice.

Yet Cotton made very little of his opportunity to wield political power either in the Commons or outside. He seems to have preferred to remain in the back rooms, useful to those at the levers of power but never seeking any for himself. Sharpe believes that, for a time, Northampton promoted Cotton for secretary, and that it was Cotton who turned it down. Perhaps Cotton was too much the expert, too little the politician; certainly, on the basis of the information presented here, it is difficult to determine either his loyalties or his principles. Sharpe makes more of Cotton's importance as a politician than the evidence he scrupulously presents quite warrants; together with that, however, he gives a very convincing analysis of Cotton's importance to the political process. It is a pity that Sharpe eschewed biography so completely: more personal detail might have helped explain Cotton's political evasiveness. Despite that, the account of the founding (and founder) of the Cottonian collection, as of the political uses made of that great armory, is invaluable.

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BUCHANAN SHARP. *In Contempt of All Authority: Rural Artisans and Riot in the West of England, 1586-1660*.

Berkeley and Los Angeles: University of California Press. 1980. Pp. xi, 292. \$17.50.

Buchanan Sharp deserves the gratitude of historians of Elizabethan and early Stuart England for what is undoubtedly the best study we have of popular disorder in this period. His book falls into two main sections: first, a national survey of food riots and government response to these disorders between 1586 and 1630; second, an analysis of motivation, leadership, and participation in the "Western Rising"—antidisafforestation riots in the royal forests of the west of England. Throughout, its unifying theme is the argument that historians have failed to appreciate fully either the significance of industrial wage labor in the economic and social structures of rural England in this period or the central place of rural artisans in popular protest. In this, Sharp is perhaps too hard on earlier students of these matters. But this is a question of emphasis. More importantly, he offers many valuable new insights. On food riots, he shows how scarcity gave way to unemployment as the principal cause of these disorders between 1580 and 1630, and he questions the significance at this time of the concept of "moral economy" as a guide to the actions of rioters. In approaching the forest riots, he provides richly detailed material on the nature of the forest communities and convincing reinterpretations of both the process of disafforestation (which was far from arbitrary in its nature) and the organization of resistance (which involved neither coordinated planning, common leadership, nor significant gentry involvement). In addition, he offers salutary reminders that riot in this period could and did express bitter class hostilities, that the authorities could experience major difficulties in responding to riot and punishing participants, and that the grievances motivating riots were much closer to the hearts of the common people than either the constitutional or religious issues of the English Civil Wars.

On the debit side, one hesitates to accept Sharp's characterization of all the forest riots as "artisan" when some of them involved village tradesmen rather than industrial workers—surely a distinction needs to be made here. Nor can one accept wholeheartedly the assertion that artisans were peculiarly prone to riot when no consideration is given to the many enclosure riots that took place in wholly agricultural communities elsewhere in England. Artisans were undoubtedly foremost in food riots and were prominent in forest disorders, as Sharp demonstrates. This is not, however, the full tally of popular disorder in this period: elsewhere yeomen were not always satisfied by compensation, and husbandmen were far from submissive. These are matters for further discussion. The fact remains that this book

is a major contribution, meriting the careful attention of all students of the period.

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R. T. KENDALL. *Calvin and English Calvinism to 1649*. (Oxford Theological Monographs.) New York: Oxford University Press. 1979. Pp. xii, 238. \$34.95.

This book will be the source of frustration and disappointment for readers with a general interest in the cultural history of England in the sixteenth and seventeenth centuries. It assumes considerable prior familiarity with both the individual thinkers dealt with and the subtleties of the predestinarian theology they espoused. By the same token it will be welcomed by the serious student of English Protestant thought. The issues treated are of central importance, and the conclusions reached may well modify previous assumptions about Puritan theological development in England.

One need read only the introduction to discover that, the title notwithstanding, the study really deals with the English *departure* from the thought of John Calvin. "Westminster theology hardly deserves to be called Calvinistic," R. T. Kendall concludes (p. 212). The evidence adduced, however, is persuasive. Kendall's approach is to trace the evolution of the doctrine of faith from William Perkins through a dozen of his most prominent followers, including John Cotton, Thomas Hooker, Jacobus Arminius, and William Ames, to the Westminster Assembly of divines. This is carried out within the context of the thought of John Calvin and his successor at Geneva, Theodore Beza.

Kendall's most important contribution is his differentiation between the theology of Calvin and that of Beza on the doctrine of the atonement—the former holding that Christ died indiscriminately for all men, the latter that he died for the elect only—and his conclusion that Perkins and his followers through the Westminster Assembly drew largely from Beza. Perkins's attempt to combine Beza's doctrine of a limited atonement with Calvin's doctrine of faith as a persuasion in the mind, however, is judged to be a failure. Perkins's followers, styled "experimental predestinarians," are credited with the resolution of the problem, achieved by making faith explicitly an act of the will and dropping Calvin's notion of temporary faith. Such a shift is effected, however, at the expense of further undermining the influence of Calvin's thought. Westminster theology, which represents the culmination of the process, is summarized as "predestinarian, voluntaristic, and experimental" (p. 198).

One of the most fascinating aspects of Kendall's study is the revelation of the close similarities between Westminster theology and Arminius's doctrine of faith. Although designed to be a creed that would counter the Arminian view of predestination, it is a "crypto-Arminian doctrine of faith that pervades Westminster theology" (p. 209).

The book includes a thorough bibliography and helpful indexes.

O. T. HARGRAVE
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MARGARET A. JUDSON. *From Tradition to Political Reality: A Study of the Ideas Set Forth in Support of the Commonwealth Government in England, 1649-1653*. (Studies in British History and Culture, number 7.) Hamden, Conn.: Archon Books, for the Conference on British Studies and Wittenberg University, Springfield, Ohio. 1980. Pp. x, 121. \$13.50.

Margaret A. Judson's *Crisis of the Constitution* (1949) has been a standard work virtually since its first appearance. Indeed, many seventeenth-century historians would regard it as still providing the best available account of how English political thought developed in the context of political practice from the accession of James I to the middle of the Civil War. The present work is not exactly a sequel to it; for the author has passed over the years 1645-49, and she deals specifically with those theorists who advanced arguments for accepting the rule of the English republic after the trial and execution of Charles I and the abolition of the monarchy and House of Lords. As she explains in the preface and again in the notes, much of the work on which it is based was done back in the early 1950s, and Judson has had to take account of the work of younger scholars who have dealt with some of the same topics during the intervening period, notably Perez Zagorin, John M. Wallace, and Quentin Skinner. Her subject has become more clearly delineated, but it has perhaps for the very same reason grown harder to say anything very original, at least on some of its aspects.

This is a study divided by types or modes of argument, not by sequence or author. Chapter 1 deals with the conflict between the monarchical tradition in English history and the passionate desire for internal peace and an end to civil strife. Chapter 2 is concerned with the scriptural and providentialist cases for submission to the government in being. Chapter 3 takes up the more novel theme that some contemporaries were developing an idea of England as a commonwealth, or what we might today call a state or nation, to which a loyalty was owing over and above that owed to any particular government,

be it monarchy, commonwealth, or otherwise. Chapter 4 relates to the sheer necessity of government, and thus the acceptance of its de facto occupants and upholders. Chapter 5 turns to the protection of particular interests by government and to the administration of impartial justice between private parties, in return for obedience.

As one would have expected, the notes and bibliography show this to be a thorough, soundly based analysis. As to its originality, almost all the primary sources cited are included by Wallace in his essay, "The Engagement Controversy, 1649-1652: An Annotated List of Pamphlets" (*Bull. N. Y. Public Library*, 68 [1964]: 384-405), although the scope of Judson's work is both wider and narrower than his. Her conclusion is that, in justifying obedience to the Commonwealth, English political thinkers turned from arguments based on constitutional tradition to those founded in political reality. What one wishes that she had considered more fully is how lasting an effect these writers—other of course than Hobbes—had beyond the limited context of the years 1649-53. Were their arguments totally abandoned after 1660, and were they copied or adapted in 1689?

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JOHN T. EVANS. *Seventeenth-Century Norwich: Politics, Religion, and Government, 1620-1690*. New York: Clarendon Press of Oxford University Press. 1979. Pp. xii, 346. \$34.95.

The interaction of local community and central government is one of the pervasive themes of the history of early modern England. John T. Evans's study is a valuable contribution to the growing literature investigating this phenomenon. Not the least of its valuable features is the chronological framework employed. The period emerges as one with a noticeable degree of unity with two major sources of instability prevailing throughout: the impact of religious Nonconformity on civic politics and the intervention of extramural authorities into local affairs. The case of Norwich provides a clear instance in which the more traditional terminal dates of 1640 or 1660 would be misleading and would obscure important elements of continuity linking the Civil War period with that of the Glorious Revolution.

In contrast to the pattern of insularity identified in many local community studies, Norwich appears to have been as intensely politicized as any other English city outside of London. A number of factors, all exhaustively examined by Evans, would seem to lie behind this marked difference. The de-

cline of town oligarchy was certainly one, as was the presence of a large and active citizenry possessing the constitutional right to participate in electoral politics and already practiced in the exercise of this right in opposition to the magistracy before 1640. Likewise the continuing presence of Nonconformity and the existence of close personal and business connections with London played a role, as did the fact that Norwich was the seat of a bishopric.

Two cautions might be entered about the picture of political life presented by Evans. First, he seems at some points rather quick to apply national "party" labels to local struggles in exact correspondence. Second, it is obvious that there were men active in Norwich politics who defy consistent party labeling; they appear at times to be in the position of holding the balance, and, though the information on them appears scanty, one suspects that they represent that "moderate-neutralist" group identified in other towns, which did indeed put local considerations first.

Two of the most interesting aspects of Evans's analysis involve the nature of the governing class and its attitude toward exterior authority. Evans fully demonstrates the lack of social or economic division within the governing class. Conflict abounded, but it was a conflict that was ultimately ideological, not social or economic; the battle raged over personnel, not over the structure of the government. Evans also makes clear that the citizens and corporation of Norwich were rarely united in their opposition to external intervention; the local faction that stood to benefit was likely to encourage and welcome it. It was one reflection of the active political life of the city that predominance of party and ideology could triumph over concerns about local autonomy.

Evans's book is a thorough and careful study. Although it is not easy reading, it greatly increases understanding of the interaction of local and national affairs in seventeenth-century England.

ROGER HOWELL, JR.
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cerning his loyalty to Parliament. Such suspicion was unjust, but the author shows that Holles displayed an unrealistic belief in the king's willingness to compromise, which weakened the peace party and deprived it of an effective policy.

As head of the political Presbyterians, Holles briefly dominated the Commons but was then cast aside by the army and Independents as a Royalist sympathiser interested only in monopolizing power for himself and Parliament. He seems to have had little concern for general grievances but to have been intent primarily on keeping authority in the possession of wealthy men like himself and out of the hands of the Independents whom he considered propertyless upstarts. Crawford sees Holles as a conservative who upheld the rights of Parliament but "believed that only by agreement between Charles and Parliament might the kingdom be settled" (p. 173). Thus he was consistent in welcoming the restoration of Charles II and in serving him as privy councillor and ambassador; for as one of a group of Whig leaders in the House of Lords he also tried to set limits on royal authority.

While the author acquits Holles of inconsistency and opportunism, she recognizes that he was often too hot-tempered and impatient to be an effective politician. (He once challenged an opponent and then pulled his nose when he refused to fight!) Though too self-righteous to win the affection of his contemporaries, Holles finally gained their respect because "his conservative ideals of a balance between King and Parliament, return to the old and known ways, were those accepted with relief by the nation at large at the Restoration" (p. 220). The book is rather brief for such an important figure, and sometimes one feels that the author might have probed more deeply into his character and motivations. Nevertheless, it is a well-written, successful study that incorporates the work of leading historians like Conrad Russell, David Underdown, and Valerie Pearl.

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PATRICIA CRAWFORD. *Denzil Holles, 1598-1680: A Study of His Political Career*. (Royal Historical Society Studies in History Series, number 16.) London: The Society. 1979. Pp. viii, 243. \$23.30.

Patricia Crawford's biography of Denzil Holles traces the career of a man who occupied a position of prominence for over fifty years. In 1629 he held the speaker down until the Commons passed the Three Resolutions. In 1640-41 he was an ally of John Pym in attacking the king's prerogatives. After the outbreak of war, however, he became so ardent an advocate of peace that he aroused suspicion con-

JOHN LANGTON. *Geographical Change and Industrial Revolution: Coalmining in South West Lancashire, 1590-1799*. (Cambridge Geographical Studies, number 11.) New York: Cambridge University Press. 1979. Pp. xiii, 322. \$44.50.

This is a commendably detailed and carefully researched study of a small coalfield some twelve miles square, in which the author offers explanations for the location of collieries and varying colliery sizes between 1590 and 1799. The crucial importance of proximity to markets and navigable waterways in determining the size and density of

the units of production is brought out; and John Langton demonstrates that the coal consumption of the average household was considerably higher in this region than in London. Industrial consumption and the export trade became increasingly important during the eighteenth century, but domestic consumption also increased concomitantly with population in this heavily industrialized area.

The book is marred by a jargon-ridden introduction on methodology and by a cavalier treatment of statistics based largely on admittedly intelligent guesswork, so presented as to give it a spurious authenticity. Some of the most important figures quoted, forming the basis of estimates of growth, are so improbable as to demand explanations. Hewers at Orrell Colliery, for example, are credited with an average annual output of 1,357 tons for the period 1788–99. How did they manage it? How many days a year and how many hours a day did they work, and with what remarkable equipment?

Another weakness is dealt with by the author thus: "The stress laid upon the spatial complexity of the economic structure of the coalfield, the cleavages between different parts of it, the variety of measure systems, markets, economic growth rates and prices, may seem misplaced. After all, the area measures only about twelve by twelve miles. A motorist can cross it today . . . in about a quarter of an hour. . . . To stress the diversity of the area and to subdivide it into two 'regions' in the seventeenth and early eighteenth centuries may seem at first sight to be an idiosyncratic product of geographical method rather than an objective appraisal of historical reality. But this smallness is a product of contemporary perceptions . . . grounded in rapid transit. . . . In the seventeenth and eighteenth centuries transport was slow and expensive" (pp. 240–42). He then produces a map of the coalfield transformed in area according to the time taken to travel a mile in the eighteenth century. This map is then superimposed on a map of the British Isles, which shows this small coalfield covering an area that takes in about half of England and Wales, southern Scotland, half of Ireland, the Irish Sea, and a slice of the Atlantic. This may make sense to a historical geographer, but it makes none to me.

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J. A. DOWNIE. *Robert Harley and the Press: Propaganda and Public Opinion in the Age of Swift and Defoe*. New York: Cambridge University Press. 1979. Pp. xi, 232. \$28.50.

In 1967 important books by Geoffrey Holmes and John Plumb restored parties, issues, and public

opinion to the central place in the study of early eighteenth-century English politics, a place from which they had unfortunately been banished for a decade by the diligent but wrong-headed Namierite studies of Robert Walcott. In 1968 William Speck's *Tory and Whig: The Struggle in the Constituencies, 1701–1715* added further depth and strength to the new interpretation of the period. J. A. Downie is a student of Speck, and therefore his first book is to be welcomed not only for its own sake but also as part (we may hope) of a new generation of special studies that will fill out, and no doubt also refine, Holmes's and Speck's broad picture.

As Downie explains, the lapse of the Licensing Act in 1695 was followed by a period of intense party conflict punctuated and heightened by no fewer than ten general elections between 1695 and 1715. The result inevitably was an unprecedented outpouring of political propaganda. It was in this period that the career of Robert Harley reached its zenith. Downie has read widely in the pamphlet literature and in Harley's unpublished papers, and he gives us the most thorough account to date of Harley's employment of propaganda and his dealings with political journalists such as Daniel Defoe, Jonathan Swift, John Toland, and a number of lesser inhabitants of Grub Street.

What made Harley a "pioneer" in Downie's view (p. 190) was his attention to propaganda and public opinion *while in office*. Most other politicians of that time turned to the press and public only during election campaigns or while in opposition. Once they had a majority in the House of Commons they rarely troubled themselves about opinion "without doors"; Sidney Godolphin is Downie's case in point. Harley, by contrast, was concerned about public opinion and propaganda whether in office or out; he also saw the value of the press, in an age before parliamentary whips, for supplying arguments and adrenalin, if need be, for his own supporters in both houses.

Downie's main contention, which makes Harley truly a hero for him, is that Harley made a crucial long-run contribution to freedom of the press in Britain. When the propaganda battles grew warm, some politicians wanted to reinstate prepublication licensing. Harley, however, was more thick-skinned toward opposing pamphleteers, and more confident as well: even when lord treasurer he preferred counterattack to suppression, and he left to the succeeding age a tradition of free discussion that even Sir Robert Walpole had to accept.

The chief faults of Downie's book derive from its origin as a Ph.D. thesis severely focused on Harley's propaganda, while assuming on the reader's part a detailed knowledge of the rest of the political picture. The writing is not always smooth and clear, and at times an excess of detail makes for hard go-

ing. All told, however, this is a sound and valuable book.

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WILLIAM THOMAS. *The Philosophic Radicals: Nine Studies in Theory and Practice, 1817-1841*. New York: Clarendon Press of Oxford University Press. 1979. Pp. ix, 491. \$39.50.

It is a pleasure to report on a book that delivers more than it promises. William Thomas makes the most modest claims for his work. The subtitle, he says, is meant to suggest that the book is neither a comprehensive study of the ideas of Philosophic Radicalism nor a continuous history of the group of Philosophic Radicals, but rather a series of discrete studies of persons and events. And the terminal date was chosen because it was then that the movement dissolved, having proved itself a "political failure" (p. 3).

In fact, the book is not as episodic as the author pretends. Most of the essays follow logically and even chronologically upon each other, although each does have a different focus. And concealed in his disclaimers are important theses about Philosophic Radicalism and the Philosophic Radicals. If the book is not comprehensive, it is because the doctrine itself was not as comprehensive, systematic, or consistent as is implied in the usual "isms" (Philosophic Radicalism, Benthamism, Utilitarianism). Similarly, the episodic treatment reflects the fact that the group was less homogeneous and united than it seemed to some contemporaries and to many historians. Moreover, the terminal date, signifying its failure, is an implicit refutation of those who have attributed to Philosophic Radicalism a large (a decisive, some would say) influence in the history of nineteenth-century reform.

Only occasionally does the author venture upon generalizations such as these. For the most part these "theses" (my word, not his) emerge negatively, so to speak. Each of the essays—on Jeremy Bentham, James Mill, John Stuart Mill, John Arthur Roebuck, Joseph Parkes, Albany Fonblanque, Lord Durham, and George Grote—reveals so many differences of principle, attitude, rhetoric, and policy that one comes away with the impression of an amorphous group, united only in its efforts for reform of one kind or another and in its conviction (until 1841, at any rate) that it could best attain those reforms by remaining independent of the Whigs and Tories.

Even James Mill, who is conventionally represented as the spokesman for Bentham, the devoted disciple (sometimes ill treated by his master, but devoted nonetheless) who undertook those tasks of

popularization and organization for which Bentham was clearly unfitted, appears here as more independent both in his ideas and his activities than has been commonly supposed. It may be said that this conclusion is flawed by the fact that it derives largely from an analysis of Mill's *History of British India*, that his *Essays* would reveal a more orthodox Benthamite, and that such deviations as appear in the *Essays* were clearly meant to make Benthamism palatable to the public. But whatever truth there is in this criticism (and I think there is some), it is also true that the conventional image of Mill has to be considerably modified, that he was, as Thomas says, more puritanical and moralistic, less rigorously utilitarian and hedonistic, than Bentham himself.

Thomas devotes more attention to the public careers and activities of these men than to their writings and ideas. In the case of most of his subjects this approach is clearly warranted, but for others it is not. If John Stuart Mill lived, as Thomas says, in a "world of ideas" (p. 200), his two main productions in this period, the essays on Bentham and Coleridge, deserve something better than the cavalier treatment they receive here. A more careful analysis would reveal them to be far less Benthamite than Thomas suggests, would make Mill himself a more interesting and complicated thinker, and would make Philosophic Radicalism more significant and at the same time more problematic.

Whatever qualifications one may have about this or that part of it, the book itself, in spite of its modest pretensions, is a major contribution to the subject—not a book to be read by the general reader (it assumes too much by way of knowledge as well as interest) but by any scholar dealing with any aspect of Philosophic Radicalism or of the period in which it briefly flourished.

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FRANK WHITSON FETTER. *The Economist in Parliament: 1780-1868*. Durham, N.C.: Duke University Press. 1980. Pp. xii, 306. \$18.75.

The political activities of sixty-two economist members of both houses of Parliament during the heyday of classical economics is the subject of Frank Whitson Fetter's book. It developed out of his studies of early nineteenth-century monetary controversies, and, not surprisingly, these topics receive the fullest and most satisfactory treatment here. In addition to their views, speeches, and votes on monetary issues, the parliamentary economists are examined on such matters as international trade, business regulation, taxation, poor relief, banking, education, religious disputes, human rights, imperialism, and electoral reform.

Unquestionably, the entry into the parliamentary arena of men versed in the tenets of the new science of political economy had some impact on the legislative process. It made possible a more rigorous and coherent analysis of the manifold problems of the British empire, if not necessarily more enlightened solutions to them. While historians have been generally aware that some such change did take place, the topic has not been systematically studied before. Although Fetter's book is thus to be welcomed, it is vitiated by two major flaws—the arbitrary nature of his classification and the failure to take into account the full complexities of parliamentary dynamics.

Moving beyond a small circle of obvious experts like Ricardo and Torrens, very few of the individuals selected were professional economists, as the author admits. They were lawyers, landowners, clergymen, journalists, merchants, manufacturers, and bankers. Undoubtedly, they had more than a nodding acquaintance with the principles of political economy, but it is going far beyond the evidence to suggest that this interest gave them an intellectual locus of primary importance in determining their attitudes toward legislation. What are we to make, for example, of the inclusion of leading politicians like William Pitt the Younger, Lord Althorp, and Sir Robert Peel? Pitt's attachment to the doctrines of Adam Smith is well known, yet the maintenance of his position among the conservative social forces that kept him in power was clearly a much stronger motive in shaping his policies. Althorp was first and foremost an aristocrat with an unswerving dedication to maintaining the ascendancy of the landed interest. Peel, as leader of his party, operated within a complex web of considerations, among which economic theory was a relatively minor concern.

Recent works (notably absent from the author's bibliography) on such major pieces of legislation as the 1832 Reform Act, the New Poor Law, and Corn Law repeal have demonstrated that theoretical formulations played less of a role than was formerly believed. Fetter's analysis of individuals supposedly linked by a common intellectual interest, without sufficient regard to their social, professional, and party identities, results in a largely meaningless pastiche of parliamentary speeches and votes that tends to obscure rather than clarify our understanding of the impact of classical economics on politics.

ANTHONY BRUNDAGE

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JOSEPH P. CHINNICI *The English Catholic Enlightenment: John Lingard and the Cisalpine Movement, 1780-1850*. Shepherdstown, W.Va.: Patmos Press. 1980. Pp. xii, 261. \$24.95.

This major study of the impact of the Enlightenment on English Catholic thought argues a new thesis concerning the origins of liberal Catholicism. By centering on the work of the historian John Lingard, Joseph P. Chinnici is able to focus on the Lockean assumptions that were so basic to the intellectual climate of the Cisalpines, the numerically minute but intellectually and politically vocal group of Catholics who were convinced that religious pluralism and freedom of inquiry were in fact allies, not opponents, of the English Catholic Church. Chinnici documents well the almost naive faith in human rationality that was shared by the Lingards, John Kirks, Charles Butlers, and their circle.

John Lingard, whose *History of England* was the most popular national history before John Richard Green's, is the centerpiece of this analysis, as indeed he was for much of his long life, which spanned the seventy years from the Enlightenment to Newman's Second Spring. Lingard began his lifetime of historical research and writing with a religiously apologetic motive but ended by producing a work remarkably objective, detached, and scholarly. In fact, Lingard's "greatest characteristic—strict adherence to documentary sources—was also in some respects his great limitation" (p. 116). So cautious was he of making historical judgments that might be interpreted as showing the bias of a Catholic priest, that a broader, thematic picture of England and its history fails to emerge.

Particularly interesting is the analysis of Lingard's liturgical views. His emphasis on congregational participation, his use of some English as well as Latin, and his criticism of "Roman devotional practices" all demonstrate Lingard's apologetic and political awareness of the position of English Catholics before the Emancipation of 1829 and the restoration of the hierarchy in 1850. He and his fellow Cisalpines were always unmistakably English.

This is a wholly admirable work. Chinnici has thoroughly used the primary sources, including the letters of Lingard at Ushaw College and the widest variety of contemporary letters, documents, and printed publications. He has, perhaps for the first time, given adequate recognition to the French influence, especially on Lingard. He relates convincingly the Cisalpine movement to the rest of both English life and Catholicism, as well as showing Lingard's central role in this small but influential group. Finally, Chinnici avoids Lingard's shortcoming with a warmer, more appealing style. In short, this study makes clear how curiously pertinent to the historiographical, theological, and liturgical debates of the twentieth century are the works of Lingard and his associates.

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I. J. PROTHERO. *Artisans and Politics in Early Nineteenth-Century London: John Gast and His Times*. Baton Rouge: Louisiana State University Press. 1979. Pp. xi, 418. \$30.00.

If E. P. Thompson's *Making of the English Working Class* tells us very little about factory workers and a great deal about artisans, I. J. Prothero is not the first to notice, but no one until now has argued its implications so insistently and with such an impressive array of research to underpin his analysis. The changing experience of the artisan, and the development of artisan politics, he argues, is at the core of working-class development in Britain in the first half of the nineteenth century. They set the pace in organizing, they set the tone in political ideas, they dictated the distinctiveness of the anticapitalist critique that hesitantly emerged. The artisans' role was derived from their experiences and craft traditions, and that theme lies at the heart of this impressive and long-awaited book.

London artisans were faced with a variety of economic pressures, some short-term fluctuations of a new kind, but others implicit in the commercial character of British industrialization as it affected the older crafts. If the analysis of these economic pressures is given far too little space in the book, Prothero nevertheless amply justifies his argument that these London artisans built their understanding of economic and political changes, and their attempts to control them, upon their own vigorous traditions of craft pride, expectations, exclusivity, and their right to get a reasonable living and a respectable independence from their trade. Through an almost bewildering variety of trade unions, inter-trade associations and campaigns, and political agitations, we see Prothero's artisans seeking to achieve these ideals in the face of increasing pressures. He weaves the elements together well. A complex fabric of craft values and Painite political radicalism emerges, within which artisans developed a critique of political economy and of their own situation that was by the 1820s beginning to mix issues of worker organization in relation to employers with demands for the political representation of *labor*. Its anti-capitalist elements, however, remained far more a rejection of the merchant middleman than of the employer of labor.

This is all subtly and persuasively presented and is the best analysis that we have of those elements of traditional political radicalism in Britain that are so vital to an understanding of the character of working-class political development. They also open up important linkages to mid-Victorian artisan consciousness, for in showing the distinctively artisan lineage of these mid-Victorian artisan values, Prothero confirms the trend of recent research on that later period.

Major aspects of early working-class development are analyzed in impressive detail and will be indispensable for both research and teaching in these areas, even if the attempt to use John Gast, radical shipwright, as a thread of continuity in the book is not entirely successful. Yet if the overall argument is both clear and persuasive, the parts remain in a frustrating fashion greater than the whole. This is a very detailed book, and the flow of argument often suffers. That is sad, because the footnotes reveal an exceptional depth of reading, especially in the radical and trades press, and this material is handled with great assurance. So, when it surfaces, is the argument, but there are long sections of the book when the reader cries out for some larger analytical presentation. The achievements of this study nonetheless far outweigh its limitations. In his depth of reading and meticulous reconstruction of very complex developments, in his careful picture of the changing character of artisan ideas, and in his detailed contribution to the history of the early working-class movement in England, Prothero has produced a valuable and indispensable book. It deserves to be read widely.

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PAUL MCHUGH. *Prostitution and Victorian Social Reform*. New York: St. Martin's Press. 1980. Pp. 306. \$27.50.

When the author of this thorough and judicious political history of the agitation to repeal the Contagious Diseases Acts first came across a reference to the regulatory code for prostitutes in Britain's garrison and seaport towns, he was admittedly mystified. Given the history of the Acts, this seems appropriate. Parliament was so reluctant to discuss the compulsory examination of prostitutes that it virtually smuggled the Contagious Diseases Acts through between 1864 and 1869 under the legislative title usually applied to measures dealing with foot and mouth disease.

Nor has the campaign of the "new abolitionists," as the opponents of state regulation styled themselves, been the subject of decisive historical analysis. There is no shortage of popular feminist accounts concentrating on Josephine Butler's charismatic role in the success of the movement. But the repeal movement had other important facets as well. It was central to the history of nonconformity, and it forms one of the most compelling episodes in the story of that characteristic element in Victorian political culture, the campaign to influence law through public opinion.

Given the abundance of archival materials, it was just a matter of time until the repeal campaign found its historians. For the political side of the

story, Paul McHugh is that historian, and his study is authoritative: on the evolution of strategy from the "nonpartisan fanaticism" of the early years to the successful attempt to influence the Liberal Party; on the place of repeal in the network of inter-related radical movements for moral and social reform; on the nuts and bolts of the complex campaign; on the frictions between north and south and men and women; on the failures of the middle-class leadership to enlist fully the support of the working classes and of prostitutes in the subjected districts. In the end, Josephine Butler, her chiliastic excesses well displayed, remains the essential catalyst, and James Stansfeld retains his position as indispensable parliamentary chief. But the little-known Sheffield radical, H. J. Wilson, joins them in the first rank.

It is a pity that there is little here about the other side of the story, the impact of the Acts on the women of the subjected districts. The author argues that his choice of emphasis "reflects the tendency amongst repealers to dismiss this sort of evidence as irrelevant to their moral attack" (p. 259). But surely the leadership's disinterest in rescue work, used here as evidence of a disinclination to bother with anything but ultimate ends, resulted as much from the knowledge that it usually failed. For the social dimension we must turn to other recent work on the Contagious Diseases Acts, especially that of Judith Walkowitz.

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DEBORAH WORMELL. *Sir John Seeley and the Uses of History*. New York: Cambridge University Press. 1980. Pp. x, 233. \$32.50.

This book is the first full account in English of the thought of Sir John Seeley. It should be said at once that Deborah Wormell has succeeded in showing how various aspects of Seeley's life and work are linked together through the influence of the Broad Church tradition and exposure to positivism.

Seeley attained notoriety with the publication in 1865 of *Ecce Homo: A Survey of the Life and Work of Jesus Christ*. *Ecce Homo* displayed Seeley's talent for syncretism, combining as it did a Broad Church point of view with the positivist belief that scientific advance constituted a new means of achieving the Christian goal of human welfare. Certainly it was as a defense of the continued vitality and relevance of Christianity that the book earned the praise of Gladstone, from whom Seeley received the post of regius professor of history at Cambridge in 1869.

Wormell stresses the role of Seeley as an educational reformer. To some extent she covers the same ground as Sheldon Rothblatt, viewing Seeley as a leader in the "Revolution of the Dons" that took teaching out of the hands of private coaches by

strengthening official college and university teaching. In addition, however, she reveals Seeley as an advocate of secondary school reform, of university extension, and of higher education for women at Cambridge. For Seeley, as for the Broad Church tradition in general, education had a quasi-religious function: it was the means through which the nation could be taught to overcome "Philistinism" through acceptance of spiritual and cultural values. This implied that teachers were a kind of lay priesthood or clerisy and that church and state were inseparable manifestations and guardians of the national identity. Although here Wormell follows the analysis set forth in R. T. Shannon's "John Robert Seeley and the Idea of a National Church" (in Robert Robson, editor, *Ideas and Institutions of Victorian Britain* [1967]), she places less emphasis on the fact that Seeley's reworking of Broad Church thought was, to cite Shannon, "practical to the point of crudity" (p. 247).

In discussing Seeley's historical writing and teaching, Wormell points out that his goal was "to create in the British people a single national consciousness with respect to the past" (p. 111). Proper understanding of the national past was to be attained through the "scientific" study of history; that is, historical facts should be used to establish inductively the laws of politics. Faith in the possibility of creating a science of politics based on historical data bespeaks the influence of positivism, yet Seeley, unlike Buckle, never attempted to put forward universally valid laws or generalizations. His historical writings were concerned with explanation of geographically and chronologically limited sets of events and with pointing out the moral or lesson that might be adduced. In *The Life and Times of Stein* (1878), for example, Seeley explained how Stein had transformed Prussia into a "moral" national community capable of fighting the "immoral" Napoleonic empire; the lesson to be learned was that in the modern state the principle of nationality was the vital source of spiritual and hence material strength. *The Expansion of England* (1833) opened with the statement that "the history of England ought to end with . . . a moral," and indeed with this book Seeley achieved his objective of influencing the manner in which his countrymen thought of their past and the possibilities of their future. Wormell focuses upon the historiographical importance of the book as well as upon its political ramifications. Through its emphasis upon imperial expansion as the salient fact of modern British history it challenged the primacy of constitutional development as the chief concern of historians. Acknowledging that the book was construed as an apologia for British imperialism, Wormell appraises the extent to which this was an accurate reading of Seeley's ideas.

Judicious appraisal, together with mastery of the sources (including Seeley's unpublished papers), are the hallmarks of what will become a standard work in British historiography. Judging by the quality of this book, the death of the author at the age of thirty-three has deprived the historical profession of a promising young scholar.

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JAMES E. CRONIN. *Industrial Conflict in Modern Britain*. London: Croom Helm and Rowman and Littlefield, Totowa, N.J. 1979. Pp. 242. \$25.00.

James E. Cronin has used the computer to prove that history is complicated. His book would be welcome for that if for no other reason, since too often statistical analysis leads to the construction of models so simplified and therefore so pat as to be of little use to historians. Cronin argues against what he perceives as a general trend in the writing of British labor history: the tendency to focus on continuity and steady development, rather than on the "oddly fitful and jerky pattern" that he believes more accurately describes the events of British labor's past hundred years (p. 56).

Cronin provides the reader with useful summaries and his particular criticisms of various theories concerning the nature of modern British industrial conflict. He then proceeds to construct a theory of his own. It is based on the proposition that one cannot speak of a single pattern of conflict and that the unevenness of that conflict is the result of the unevenness of economic growth. Each "wave" of strike activity, Cronin argues, differs significantly enough from others to warrant independent analysis. The components of Cronin's model, therefore, are (1) the constantly changing and hence non-linear form, meaning, and context of strikes over time; (2) the close relationship between those changes and the economy; and (3) the "mediating roles assigned to workers' consciousness and organization" (p. 74). He tests his hypothesis with brief but lucid discussions of various key periods (1889-90, 1910-13, 1919-20, 1957-62, 1968-72) and by a seemingly straightforward (though, to this non-initiate, difficult to comprehend) multivariate analysis. In the process he assigns weight to such factors as the standard of living, generational conflict within the working class, the effect of technological change, the pressures compelling workers to strike first and frame demands afterwards—in short, the pieces that any intelligent historian must try to put together into some sort of coherent form.

One is left wondering, however, how other model builders will react to Cronin's fluid patterns. His

willingness to accommodate to change and complexity, though it should please many historians—as it pleases me—may strike the more rigorous social scientists among the profession as unnecessarily untidy. My own disappointment with the book stems from Cronin's failure to discuss more fully his assertion that the strikes he has studied were generally "creative acts of an offensive kind, signs . . . not of weakness but of collective resources" (p. 9). The point is certainly worth debating; but Cronin, having made it, spends little time developing it explicitly. To say, as he does, that the working class is being constantly made and remade as changing economic conditions demand new responses to new problems is not the same as arguing that those responses are more offensive than defensive. Cronin believes in "big, broad explosions of *creative militancy*" (p. 194, my italics). But, despite his devotion to careful statistical analysis, he seems to me to take this particular conclusion too much on faith.

STANDISH MEACHAM
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Austin

KLAUS WORMER. *Grossbritannien, Russland und Deutschland: Studien zur britischen Weltreichpolitik am Vorabend des Ersten Weltkriegs*. (Veröffentlichungen des Historischen Instituts der Universität Mannheim, number 6.) Munich: Wilhelm Fink Verlag. 1980. Pp. 397. DM 78.

Klaus Wormer's new book treads well-covered ground. It is an account of British foreign and imperial policy in the seven or eight years before the outbreak of World War I. He makes some use of Foreign Office dispatches, and much is derived from the extensive and still growing secondary literature on the period. Still, little new material emerges to change the outlines of a reasonably well-known story. The author's contribution is an interestingly argued synthesis, which weaves together both domestic and external factors in a manner that clarifies the fundamental foundations of British foreign policy at the time.

Wormer characterizes British policy as one of seeking an acceptable balance of power on the Continent while preserving European peace. But when an attempted German hegemony over Europe was perceived by London, Britain began to move from a position of isolation toward commitment, especially, Wormer argues, as growing pressure upon the British empire and domestic problems such as the lack of money, Ireland, and social reform reinforced the effect of an external threat from Berlin. When the empire faced increasing foreign challenges at the end of the nineteenth century, the government found that it lacked the military and naval

power to secure widely scattered imperial interests. The result was a change in tactics, which was manifested not only in the two ententes of 1904 and 1907 but also in the signing of an alliance with Japan in 1902.

The Japanese alliance brought some relief for British naval forces in the Pacific and permitted more concentration of the fleet in European waters. Similarly, the entente with France affecting world-wide empire lessened Britain's imperial military burden. After these two agreements were concluded, Wormer feels that the government could have cooperated further with either Russia or Germany. St. Petersburg was chosen because the Russian military defeat by Japan made it less threatening than Germany; France, the partner of both, was urging such an arrangement; Russia would play a major role in a defensive system against German hegemony; and, finally, the Middle East could be made secure without spending any additional money. Germany's blustering, blundering Moroccan policy and its increasing naval power also naturally played a role in pushing Britain toward Russia rather than Germany. Part of the British insistence upon its own naval supremacy was a reflection of what was considered necessary for the maintenance of empire.

There were attempts made in this period to achieve an acceptable accommodation with Germany, but they foundered upon Germany's naval challenge and the British refusal to grant Berlin naval and imperial parity. Britain's growing dependence upon its entente partners to blunt or contain the threat from Berlin (a dependence necessitated by imperial overextension and domestic problems) rather than or as much as Belgium or German economic rivalry was why, Wormer maintains, Britain had little choice but to go to war in August 1914.

RICHARD MILLMAN
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Chicago

STEPHEN WHITE. *Britain and the Bolshevik Revolution: A Study in the Politics of Diplomacy, 1920-1924*. New York: Holmes and Meier. 1979. Pp. xii, 317. \$32.00.

In his introduction to this rather uneven book, Stephen White claims he has written "a study in the sociology of international politics rather than a record of diplomatic interchange." Yet most of his account of Anglo-Soviet relations from 1917 to 1924 (with emphasis on the period following the trade agreement of March 1921) is fairly conventional diplomatic history. Moreover, it is diplomatic history so densely written, with so little attempt to inform readers about the context of particular utterances or actions, that few nonspecialists are likely to spend long with the volume.

Only in its concluding chapter, "Class, Party and Foreign Policy," does the book really come alive. There White asks who within British society sought "normal" relations with the Soviet regime. Not surprisingly, he concludes that the impetus toward the diplomatic recognition that was conveyed by Ramsay MacDonald's minority Labour government in 1924 came primarily from British industry, represented in politics by Andrew Bonar Law and Stanley Baldwin, MacDonald's two Conservative predecessors, and from former Liberal foreign-policy specialists like Charles Roden Buxton, J. C. Wedgwood, or E. D. Morel, who had defected to Labour because of the wartime refusal of Liberal leaders to countenance a negotiated peace. Bonar Law and Baldwin could not act when they were in office because they were blocked within their own cabinets by the "colonial-agrarian-finance" group led by Lord Curzon and Winston Churchill.

White is concerned to debunk the interpretation—conventional among not only Soviet but also most Western scholars—that MacDonald and the Labour leadership moved to recognize the Soviet government because of upward pressure from more radically disposed rank-and-file workers who were motivated by class solidarity with the Russian workers' state. White finds no evidence to support this interpretation; indeed, quite the contrary. The "Hands Off Russia" Committee and the Councils of Action that blossomed during the Polish-Soviet war of 1920 atrophied soon afterward: British workers sought to keep Britain out of war, not to save the Soviet regime. Indeed, so weak was their sense of class solidarity that, despite their leaders' appeals, British trade unionists contributed scarcely a farthing per organized worker to the relief of the major famine that afflicted Russia in 1921-22.

White notes that, despite exceptionally high working-class militancy on domestic issues, "even the most radical sections of the movement . . . consistently failed to discern a connection between the Russian workers' attempt to establish a socialist state and their own domestic objectives." Like Baldwin and his industrialist friends, MacDonald and his Labour cabinet were moved to normalize Anglo-Soviet relations by the lure of Russia as a market for British manufactures and as a source of cheap raw materials. The rest of the labor movement shared these goals. White deplores this absence of class consciousness. He correctly notes that workers were hardly supreme in the British Labour government of 1924, yet neither were they in Soviet Russia. British workers were undoubtedly insular and often misled. But it is difficult to conclude that in their failure to identify with the Soviet regime they were mistaken.

RICHARD H. ULLMAN
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A. G. SUDEIKIN. *Kolonial'naiia politika leiboristskoi partii Anglii mezhdurumia mirovymi voynami* [The Colonial Policy of the Labour Party of England between the Two World Wars]. Moscow: Nauka. 1976. Pp. 267. 1 r. 36 k.

No surprises here! Labour misrepresentatives (J. A. Thomas) earn ridicule; "Independents" like Lansbury win condescending praise; Ramsay MacDonald gets his just deserts. The prize goes to Britain's Communist Party—invariably correct—whose judgments and criteria startlingly resemble the author's own. Palme Dutt gives the curtain speech.

It is a story with little progression. Characters move on and off stage; their speeches remain much the same over the years, amid author's comments equally uniform. Nonetheless, chronologically five items deserve mention. (1) Until 1914, Labour spokesmen endorsed Britain's "tutelary" role. (2) Radical upsurge (1918–20) among the masses, influenced by Asian and African militancy and by Soviet example, forced Labour leaders into bold (and embarrassing) promises for the future empire. (3) Betraying these promises, both Labour governments kept continuity with their predecessors. (4) Theory turned back in the 1930s toward the right, at least among leaders. (5) Judged by flexible adaptation to events, many Tories did better than Labour's leaders from 1929 on.

More broadly, A. G. Sudeikin alleges (1) British workingmen did not—as alleged—endorse imperialism. They could declare radical support for colonial peoples' struggles, and at least once (the *Jolly George* incident) influenced policy. (2) Though some workingmen benefited objectively from imperialism, British labor in general was hurt by the empire—which divided the workers of colonies and Britain whose unity alone could overthrow capitalism and provide a real solution. (3) Like the "commonwealth" goal, proposals to "socialize" the empire were utopian at best. Total separation from the empire was the only solution—but was never offered; Labor stopped short, offering only "self-government" within the empire.

Overlooking faults typical of "Nauka" books (reliance on sheer repetition, lopsided quoting from Communist Party periodicals, lack of serious argument, let alone proof), Sudeikin's major handicap may be this fixed emphasis on separation/independence. He seems unaware that, for many Labourites, home rule may have been preparatory to, or interchangeable or compatible with, independence. Change that assumption, and one challenges all. Serious, too, is his dismissal of concern for "minority rights guarantees."

Perhaps a book in which Haldane is declared culprit or originator of World War I is less than serious. Still, it was cheering to read Sudeikin's sturdy

denunciation of aircraft sowing death, from the skies, on civilians—good point.

PAUL B. JOHNSON
Roosevelt University

ALEXANDER MURDOCH. *"The People Above": Politics and Administration in Mid-Eighteenth-Century Scotland*. Edinburgh: John Donald Publishers; distributed by Humanities Press, Atlantic Highlands, N.J. 1980. Pp. xi, 199. \$30.00.

In the past two decades Scottish nationalism and other discontents with the relationship between Scotland and England have played a significant part in the political life of the United Kingdom. It is not surprising that historians, most prominently P. W. J. Riley and William Ferguson, have begun to produce detailed studies of the origins and evolution of the unique constitutional, political, and administrative arrangements inaugurated in 1707. These studies represent a radical departure from most other recent work on eighteenth-century Scotland that has emphasized economic, cultural, intellectual, and social history. Alexander Murdoch has consciously sought to supplement Riley's publications on the first part of the century with a political analysis of the period between the '45 and the rise to dominance of Henry Dundas in the 1780s. His subject is "the tiny electoral nation of Scotland, the political elite who owned the land, had made the Union, and sent their representatives to Westminster" (p. vi).

The first chapter is a brief but clear survey of the administration of Scotland 1707–84, containing sections on the Union of 1707, government from London, government in Edinburgh, and local government. The central political problems of the period are shown to be the absence of an official Scottish executive, the continuing indifference of Westminster toward Scotland, and the lack of institutional and intellectual articulation between Scotland and England. The rest of the book is a detailed monograph describing the emergence and functioning of Archibald, third duke of Argyll, as the dominant Scottish politician, the decline of "the Argathelian system" under Lord Bute and his brother James Stuart Mackenzie, and the absence of a Scottish manager-minister from 1765 to 1780. All the usual features of eighteenth-century politics appear: patronage deals, electoral rivalries and accommodations, the predominance of personal interests. Nevertheless, some interesting and distinctive patterns emerge. Argyll is presented as representing the older Scottish elite who sought to preserve Scottishness and Scotland's semi-independence while seeking "parity" within the United Kingdom. At the same time he provided strong leadership in Scotland and

in negotiating Scottish interests with London. Bute, Stuart Mackenzie, and Dundas, in contrast, appear as examples of a new elite of Anglo-Scots whose education, culture, and careers were "British" and who saw Scotland as a place to be managed, not represented.

This is clearly a major work in its specific field. Part of its value lies in its extensive scholarly apparatus. The very full notes, at the back "safely out of the general reader's way" (p. xi), contain excerpts from the sources and extensive commentary. The bibliography is comprehensive and critical. There are also many tables, annotated patronage lists, and a political chronology. While this sort of administrative history is seldom fascinating, Murdoch has succeeded well in elucidating both the details and the nuances of Anglo-Scottish political relationships in the mid-eighteenth century.

J. WILSON FERGUSON
Russell Sage College

IAN LEVITT and CHRISTOPHER SMOUT. *The State of the Scottish Working-Class in 1843: A Statistical and Spatial Enquiry Based on the Data from the Poor Law Commission Report of 1844*. Edinburgh: Scottish Academic Press; distributed by Columbia University Press, New York. 1979. Pp. x, 284. \$15.00.

This book is a dazzling display of what the latest computerized research can do with historical raw material. The Scottish Poor Law Commission presented a massive report to Parliament in 1844 that filled six volumes and consisted of replies to a questionnaire of seventy items sent to parish officials. The response rate was 93 percent, providing sixty thousand answers, and upon them Ian Levitt and Christopher Smout set their computers to chew and grind and sort.

There are chapters on food, prices, wages, and emigration, all less interesting than the chapters on the actual operation of the harsh and variable old Scottish Poor Law. Topics such as drink, savings banks, and friendly societies serve to illustrate the tension between thrift and dissipation among workers and their families. All of these chapters have clear, well-written introductions and conclusions that sandwich great globs of minute detail.

The authors place much effort on explaining gaps and difficulties in processing the raw material of the six-volume report. Perhaps the most serious hiatus is the lack of response from some larger cities, but Levitt and Smout try to make up for this by using other sources. Unsatisfied with the old standard divisions into Highland and Lowland zones, as well as with county divisions, they drew up forty-three districts after scanning many computer-drawn maps.

The computer was also responsible for many more of the maps and graphs and charts that fill so much of this book. The detail conveyed is impressive to the point of being amusing. For instance, there is a map of peat fuel use, a map of peasemeal consumption, a map showing just where married farm servants were preferred over nonmarried ones, a map showing population per public house (shockingly low in some places), and a map indicating which parishes reported pawnshops. Sometimes the computer produces obvious and banal conclusions: we learn that drinking was the favorite recreation of Scottish workers, that suffering is really subjective, that people in preindustrial society were poorer but more equal in their poverty, and that man does not live by economic conditions alone.

Several significant overviews emerge from this study: Scotland can be seen in 1843 halfway between an undeveloped and a modern economy, actually having a dual economy. Moreover, the spatial effects of rapid economic growth in a free market economy are strikingly presented, as is the grinding, harsh old Scottish Poor Law. The authors, despite the cold calculations of their machines, have a genuine human compassion for its victims.

Levitt and Smout claim that their book presents a "photograph" of the Scottish working class in 1843. In terms of economics it does, but the photograph omits many political and social developments of Scottish workers. For instance, Chartism in Scotland is only mentioned incidentally. Rather than a photograph, the book is in many ways a careful, well-written distillation of the massive royal commission report.

HENRY WEISSER
Colorado State University

R. B. MCDOWELL. *Ireland in the Age of Imperialism and Revolution, 1760-1801*. New York: Clarendon Press of Oxford University Press. 1979. Pp. 740. \$73.00.

In the preface to his first book, *Irish Public Opinion, 1750-1800* (1944), R. B. McDowell remarked that, although Lecky and Froude had treated eighteenth-century Ireland "on a generous scale," a series of special studies was needed for an adequate understanding of the period. Since then a number of other scholarly monographs and articles have appeared as well as several valuable sources. The time indeed has arrived for a new general synthesis. McDowell's contributions have been primarily on the nineteenth and twentieth centuries, but he has written articles on the eighteenth century and edited part of the Burke correspondence. *Ireland in the Age of Imperialism and Revolution, 1760-1801* provides a careful study of many aspects of the period; it is based on an impressive array of sources, especially

newspapers and pamphlets. Nevertheless, it is not a comprehensive history of the period, comparable to Steven Watson's volume in the *Oxford History of England*. The author concentrates heavily on political history, and over two-thirds of the book is devoted to the last years of the century.

The book begins with three topical chapters on economic and social structure, administration and politics, and intellectual and religious life. The last is the most useful, supplying an informative and balanced treatment of the Church of Ireland, the Presbyterians, and the Catholic majority. The first two chapters offer less that is new and demonstrate two shortcomings that, to a degree, are characteristic of the entire study. Although the author's wealth of citations to sources is commendable, he seems inclined to treat them all as of equal weight, without assessing bias or probable accuracy. More regrettable is McDowell's limited use of references to modern scholars. The reader expects a book of this scope (and I might add price) to include a sophisticated guide to recent scholarship. It has a good bibliography for sources but contains only a relatively short list of secondary books and no articles. The footnotes fail to make up this deficiency. The first two chapters together have citations to only five articles, one of them by the author. The publications of several scholars such as Michael Drake, David Large, and A. P. W. Malcomson find no mention in footnote or bibliography, and there is no specific reference to L. M. Cullen.

In short, the book, like Lecky's classic, is essentially a critical narrative of the mainstream of Irish history. McDowell retains a traditional view of the earlier eighteenth century as an era of "political placidity" and betrays his ignorance of that period when he speaks of "the very long intervals between parliamentary sessions" (p. 209). Actually, Parliament met regularly every two years from Anne's reign until 1782. Despite this lack of background, the book offers a good summary of events leading up to the winning of the constitution of 1782. It gets better as it moves to the revolutionary decade, 1790-1800; this is clearly the epoch on which McDowell's interest and research have concentrated. He assesses Pitt's Irish policies sympathetically, particularly at the time when his coalition with the Portland Whigs led to the appointment of Fitzwilliam as lord lieutenant. Lecky believed that Fitzwilliam's subsequent recall was a fatal turning point in Irish history. A. P. W. Malcomson, in *John Foster: The Politics of the Anglo-Irish Ascendancy* (1978), implies that Fitzwilliam's radical aims were self-defeating. McDowell, although sympathetic to Fitzwilliam and his objectives, criticizes his political blunders and stresses that his failure was "to a considerable extent his own fault" (p. 457). One of the best sections of the book is on the rebels of 1798.

McDowell gives a sensitive appraisal of the dilemma that many faced when forced to repudiate pacific for violent methods. He also portrays the attitudes and policies of Cornwallis and Castlereagh with understanding and includes a thoughtful discussion of the government's use of martial as opposed to civil law.

Throughout the book McDowell draws, from his storehouse of sources, apposite quotations that enliven and enlighten the text. And these are not confined to Irish sources. He quotes from Dr. Johnson on why the Irish adapt to English ways better than the Scots and from Rufus King, the U.S. ambassador in London, who wrote regarding the exiled rebels of 1798 that he could not persuade himself "that the malcontents of any country will ever become useful citizens of our own" (p. 657). In summary, McDowell has produced a well-written and interesting study of Irish political history during the latter part of the eighteenth century. For a more complete treatment of the era we must await the fourth volume of the *New History of Ireland*.

FRANCIS G. JAMES
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RONAN FANNING. *The Irish Department of Finance, 1922-58*. Dublin: Institute of Public Administration. 1978. Pp. xxi, 707. £16.50.

Ronan Fanning is a historian of considerable talent. Unfortunately his efforts have to some extent been misspent on this lengthy enterprise. Given exclusive access to the records of the Irish Department of Finance, the author has obligingly shared this munificence by extensive quotations and the inclusion of many departmental memoranda in block quotes. Although this material is impeccably reproduced, the result is an excessively long and often tedious narrative that is more of a documentary than a history. In addition, the Institute of Public Administration, although it has published much useful information on various aspects of Irish government, has produced a less than satisfactory copy in this instance. Pages are often creased, margins skewed, and, in this reviewer's copy, some pages are missing and others included twice.

Notwithstanding these compositional and technical shortcomings, the development of finance, the premier department in the Irish government since 1922, is a story that deserves to be told. Despite its revolutionary origins, states Fanning, the department followed a conservative course derived chiefly from the policies and personnel of the recently reformed British civil service. Its retrenchment in defense estimates led directly to the army mutiny of 1924, and its cordial fiscal relations with England ran counter to the radical economic policies insti-

gated by Eamon de Valera's government after 1932. During World War II, however, the department's conservatism was vindicated to a great extent by the shortages caused by Ireland's isolated neutral stance. Afterwards the availability of Marshall Plan funds and the general need for economic recovery changed the picture somewhat, but finance continued to show restraint until the introduction of state planning and the publication of *Economic Development* in 1958.

Much of this emphasis on tradition was due to the long tenure of such individuals as Joseph Brennan, J. J. McElligott, and O. J. Redmond, who successively served as secretary of the Department of Finance and in other important capacities. Critics of the department have argued that these officials fostered a policy of stagnation, but an improved historical understanding, such as rendered here, indicates that this conservative policy was essential in establishing financial stability, which has bestowed inestimable benefits on the citizens of the Irish Free State and Republic.

This story is concluded by a useful epilogue that describes how conditions have changed during the last two decades, resulting in a more progressive Keynesian approach to government finance—just as the more advanced economies move into the post-Keynesian era. Clearly Fanning's work adds substantially to the understanding of Irish government provided by Basil Chubb a decade ago and complements the valuable study by Maurice Moynihan on currency and central banking, but a more concise text would have made it a more valuable contribution to the historiography of modern Ireland.

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JAMES B. WOOD. *The Nobility of the Election of Bayeux, 1463–1666: Continuity through Change*. Princeton: Princeton University Press. 1980. Pp. xiv, 220. \$16.00.

James B. Wood's monograph is a significant addition to the studies that, with different approaches and emphases, have begun to reorient our understanding of the ruling classes of early modern France. Wood's own approach is emphatically and exclusively quantitative. On the basis of intensive study of a small region in lower Normandy, Wood argues that the nobility maintained its position successfully through the sixteenth and seventeenth centuries. Its economic position actually improved, and it faced no serious threat from the bourgeoisie or new nobles. Old families died out less often than historians have usually thought and remained the

core of the late seventeenth-century nobility. Relations between old and new nobles were easy; intermarriage was frequent, and career patterns were similar, with old nobles often holding royal offices and new nobles serving in the military. Consolidation rather than crisis, argues Wood, was the principal theme of the nobility's early modern history.

Wood's conclusions are important and will need to be examined by most historians of early modern France, but his book also elicits reservations. Wood's treatment of other historians is usually ungenerous, often inaccurate or incomplete; among the major studies not mentioned are those of Emmanuel Le Roy Ladurie on the Sire de Gouberville, Jean Jacquart on the Hurepoix, Guy Bois on upper Normandy, and Robert Harding on the provincial governors. The book also relies heavily on a single, problematic source, the investigations that the monarchy periodically undertook in an effort to unmask false nobles and make them pay taxes. Wood offers arguments for taking these *recherches* more seriously than most historians have done, but the findings that they suggest need to be confirmed and deepened by comparison with documents that the nobility compiled independently of fiscal pressure: accounts, notarized contracts, letters, and memoirs.

Wood's reliance on a single source and neglect of previous studies are related to a tendency throughout to overstate findings and to draw unwarranted inferences from them. Having shown that Protestantism appealed to the more affluent strata within the nobility, for instance, he claims to have disproved "the more traditional materialistic explanations" (their adherents are never named) of noble political activism during the period (p. 166). Finally, the book's initial definition of the nobility is very broad. In calculations concerning the "old nobility," Wood has grouped tax officials, urban *rentiers*, and new arrivals in the region together with the established rural gentry whose fate so preoccupied contemporaries. It is perhaps not surprising that the nobility has a novel appearance when defined in this way.

Wood's book will not be the final word on the lower Norman nobility. It is nonetheless an important and intriguing study that raises questions and hypotheses that should be pursued.

JONATHAN DEWALD
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ROLAND MOUSNIER. *Les institutions de la France sous la Monarchie absolue, 1598–1789*. Volume 2, *Les organes de l'Etat et la Société*. (Histoire des Institutions.) Paris: Presses Universitaires de France. 1980. Pp. 670.

Volume 2 of Roland Mousnier's massive *Institutions de la France* on state "organs" from 1598 to 1789 is one of the most impressive institutional studies of the *ancien régime* since the French Revolution made the regime *ancien*. Like his first volume of 1974 that focused on the hierarchical society of orders during the same period, this current study is firmly placed in its social context. One should refer back to the initial volume for such political subjects as urban and ecclesiastical government, just as one finds much social analysis in the present volume—notably the massive statistical descriptions of successive governing elites' economic, familial, professional, and educational connections and the frequent allusions to the *fidélité*, *lignage*, *créatures*, and *maîtres* that bonded the society of orders. Together, the two volumes constitute an original synthesis that incorporates and goes beyond the lifetime scholarship of this distinguished French historian.

Part of the originality, demonstrated throughout the second volume, lies in the author's insistence on seeing "revolutionary" alteration over time within seemingly unchanging political structures. Mousnier has the rare ability to be as accurate and understanding about the metamorphosis of the *ancien régime* around 1789 as at the time of Richelieu. And he constantly directs us from formal institutions to living, changing practices and the actual governing groups and individuals that initiated them. Individual kings' education and style of governing, clusters of ministers at a given time, and illustrative careers of intendants all pass in review. At times Mousnier's institutional history reverts to traditional narrative; on other occasions it approaches the newest *pointilliste* historical approach, as he focuses on the Estates General of 1614, the formulation of royal commands through chancellery, conciliar, and secretarial channels, criminal torture and witch-hunting in the law courts, and even the names, careers, and ministerial connections of tax farmers. In my opinion, Mousnier's second volume is vastly superior to the many histories of French law and institutional "dictionaries," and it nicely complements the shorter and equally first-rate *Ancien régime: Société, pouvoirs* by Pierre Goubert.

It would take a full-length article to expound and comment on the basic thesis within this synthesis. Suffice it to say here that the central theme is the governmental evolution from a "judicial" administration to an "executive" one, punctuated by Louis XIV's conciliar "revolution" of 1661 that shifted power and control from the "service nobility" of *officiers* to the removable *commissaires* (including ministers and intendants). Mousnier correctly insists that the state, not economic or social factors, was the motor of these changes. He is less convincing in drawing a line between the rival elites of *officiers* and *commissaires*, who were socially, educationally, even

professionally very close, as Mousnier's elaborate statistical apparatus shows. If the *officiers* defended the society of orders while the *commissaires* eroded it through loyalty to monarchical centralism, as the author contends, their rivalry looks like a family feud within the world of the *bourgeois gentilshommes* recently studied by George Huppert. Their similarities are all the more striking when compared with a third modern governing elite, the *fonctionnaires* or "men of talent" who were devoted solely to their careers and functions rather than combining function, *rentes*, land, and dignity as did both previous elites. Mousnier's greatest contribution to scholarly debate in this outstanding synthesis may be his tracing of the origins of the third elite, described in Ezra Suleiman's *Elites in French Society*, to the late eighteenth-century *commis* in the bureaus of state secretaries, controllers general, intendants, tax farmers, and engineering corps. It would be a fitting tribute to Mousnier if his current work sparked new interest in the still mysterious shifting of governing elites from a "society of orders" to the "society of classes" with careers open to talent.

A. LLOYD MOOTE

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NICOLE CASTAN. *Justice et répression en Languedoc à l'Époque des Lumières*. Paris: Flammarion. 1980. Pp. 313.

Nicole Castan has based this important book on more than 4,600 criminal cases that came before the *parlement* of Toulouse between 1779 and 1790. With such massive evidence, the French school of historical criminology thus moves from an overview of selected materials ("par sondage") to a penetrating analysis of comprehensive data; the "serial" history of crime has finally emerged.

What makes this book so important is precisely Castan's conscious attempt to use criminal data as a basis for studying the criminal justice system rather than simply using a mass of statistics to tell us something (which we probably already know) about crime itself. In place of the banal correlations between criminal trends and price indices, harvest totals, baptismal levels, or wage rates, the author attempts to elaborate the structures that comprised the criminal justice system of the Old Regime. Lurking in the background, although never explicitly spelled out, are the Enlightenment critiques of the penal system that served as the springboard for attacks on the political structures of absolutism.

In essence, the work graphically illustrates the workings of a judicial system that was in the throes of transformation from private to public procedure; that is, in which the responsibility for adjudicating disputes was passing out of the realm of the individ-

ual and into the hands of the state. Certain elemental vestiges of the traditional process remained, most importantly the use of mediators (*arbitrage*) to avoid a formal settlement in the court. Castan's evidence is particularly instructive here as regards the type of person chosen to be the arbitrator and how this choice reflected distinct social perceptions in the urban as opposed to the rural milieu. The question of mediation was crucial to most disputes insofar as a formal procedure usually lasted more than a year and cost more than the average person could afford to spend. But if Castan believes that the problem of judicial inefficiency reflected the onset of a great crisis, a "conjoncture judiciaire" (p. 57), how would she characterize today's system in which the disposal of a criminal case in less than one year is absolutely unknown?

The book concludes with a long section on punishment, an issue whose more arbitrary and brutal aspects were key targets of Enlightenment penal reformers. Here and elsewhere, particularly the section on unreported crime, the work suffers from a lack of bibliographical context. An overly impressionistic discussion on traditional hunting rights and rural crime, for example, would have been clarified with reference to the work of E. P. Thompson. The book presents difficulties in those areas where Castan deals with the nature or context of crime. On the question of punishment, however, the evidence is compelling and nearly complete.

MICHAEL R. WEISSER
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MANUELA ALBERTONE. *Fisiocrati: Istruzione e cultura*. (Fondazione Luigi Einaudi, Studi, number 26.) Turin: Fondazione Luigi Einaudi. 1979. Pp. 211. L. 6,000.

After a rather long period of relative neglect, the Physiocrats are enjoying a considerable popularity among scholars of the eighteenth century. Various important works on Physiocracy have appeared in recent years, in America as well as in Europe, including Elizabeth Fox-Genovese's *The Origins of Physiocracy* (1976), Folkert Hensmann's *Staat und Absolutismus im Denken der Physiokraten* (1976), and James J. McLain's *The Economic Writings of Du Pont de Nemours* (1977). Manuela Albertone's contribution, which comes from Italy, a country traditionally linked to French culture, is a further proof of such revival of interest in the Physiocrats.

The first thing to be noted is the author's unusual approach to her topic. Although Physiocracy has hitherto been studied from the viewpoint of economic history, Albertone focuses her research on the educational dimension of Physiocratic thought. She considers education the central issue of this in-

tellectual movement. Economic science was conceived by the Physiocrats as the result of eternal laws of natural order, knowledge of which could be attained through Cartesian evidence. In order to guarantee the triumph of Physiocracy, a general system of public instruction, founded on utilitarian premises and capable of eradicating illiteracy from society, was strongly advocated as the only means that could ensure the diffusion of the knowledge of the *ordre naturel*. In this context, the Physiocrats stressed the importance of public opinion as a means of controlling the arbitrary power of kings.

The views on education expressed by the Physiocrats allow the author to detect a wide spectrum of diversified positions that defeats any attempt to treat Physiocracy as a monolithic movement. Albertone, therefore, has treated separately the attitudes toward education of Mirabeau, Quesnay, the *Sociétés d'agriculture* (the first one was founded by Bertin and Turgot, at Tours, in 1761), Baudeau, Du Pont de Nemours, Le Mercier de La Rivière, Butré, Le Trosne, as well as the polemic reactions of their adversaries. The best part of the book is the fifth chapter, in which the author traces an original profile of Du Pont de Nemours, a correspondent of Thomas Jefferson. The activity of Du Pont de Nemours in Poland, as secretary of the Committee on Education (1774), is amply illustrated on the basis of unpublished manuscripts, preserved at the Polish Academy of Sciences and at the Czartoryski Library. Three *Mémoires* written by Du Pont de Nemours for the same committee are appropriately edited by Albertone in an appendix. Other manuscript sources consulted in Paris are listed at the beginning of the substantial bibliography that completes this intelligent volume.

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JEREMY D. POPKIN. *The Right-Wing Press in France, 1792-1800*. Chapel Hill: University of North Carolina Press. 1980. Pp. xix, 234. \$22.00.

Jeremy D. Popkin's survey of approximately one hundred, mostly post-thermidorian and Directory, right-wing newspapers breaks ground with its excellent use of extremely rare printed collections at the Bibliothèque Nationale and Harvard Library and masterful detective work in the Archives Nationales on finances, circulation, profits, salaries, and censorship. These newspapers split into three tendencies: moderate or center right, such as Roederer's *Journal de Paris*, rarely cited by Popkin; constitutional monarchist, such as the *Nouvelles politiques* distinguished by J. B. A. Suard and Charles Lacretelle's collaboration, on which Popkin relies

heavily; and royalist, such as the *Quotidienne* for which Jean Thomas Langlois wrote articles supporting absolutism.

The French-language political press emanated from outside French borders in the eighteenth century. For all effective purposes, one can say that the French Revolution, in destroying monopolies and sporadically (1789–92, 1794–97) establishing broad freedom of press, created the conditions necessary for modern French journalism to “take off.” “No other city in the western world has ever supported so many competing newspapers for such a long time” (p. 6). Popkin puts the total Parisian readership at its peak before the coup d’état of *fructidor* at 150,000, which was reduced to 90–98,000 by repressive press censorship during the Second Directory. Although Popkin does not risk estimating the percentage of this total that was right-wing (given the questionable authenticity of circulation figures), one can surmise from the tables and appendix that right-wing journals had the heaviest circulation, whence the importance of the subject.

Popkin does not see the right-wing press as an instrument of Pitt and Coburg (only a very few were subsidized by counterrevolutionaries outside France). On the contemporary question “Do the newspapers corrupt the public spirit or does the public spirit corrupt the newspapers?” Popkin concludes that his papers followed more than formed right-wing opinion. The importance of his newspapers lies elsewhere. Chapter 2 establishes that journalism was the “‘most lucrative branch of French literature,’” and Popkin thoroughly analyzes the handsome profits that entrepreneurs and journalists could reap in a new free-market economy in newspaper publishing.

The organization of Popkin’s book is admirable, and chapters 5–8 on social theory, constitutional government, international order, and the legacy of this press will be of interest to students of political thought and of the history of ideas. Just as Popkin disassociates his newspapers from Pitt and Coburg, so he disassociates their social, political, and religious thinking from Burke, De Maistre, Bonald, and Chateaubriand. Instead he finds they were more indebted to the Enlightenment, which they used for their own purposes. Generally, the right-wing journalists wanted a state (preferably Catholic but not ultramontane) religion. They repudiated the providential explanation of the French Revolution, often advocated a constitutional monarchy, denied De Maistre’s claim that war is the natural condition of mankind, and stood out as the peace party of the Directory. This right wing, Popkin argues, contributed much to the configuration of nineteenth-century politics in its legitimist or Orleanist strands, independently of the 1815 victory of the counterrevolution.

Three criticisms could be made of this work. First, Popkin tends to use pamphlets by Lacretelle and others to help document the tendencies of their newspapers, which seems questionable. Second, although Popkin claims there were over one hundred newspapers at one time or another between 1792 and 1797, only seventy-two are listed in the bibliography and only fifteen are cited more than five times in the footnotes. In the ultimate analysis, the profile of the right-wing press is largely based on a somewhat limited sample. Opinions supposedly representative of a large segment of the press are usually documented by one newspaper reference. In dealing with such an enormous corpus, multiple citations would have been highly desirable. Finally, Popkin’s statistical methods, used so successfully in his analyses of readership distribution and profits, could be applied in the manner Jack Censer has done to a quantitative analysis of content. For lack of this, one is left with a somewhat impressionistic, but nonetheless extremely valuable, account that will not be surpassed easily or for a long time.

EMMET KENNEDY

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SUSANNE PETERSEN. *Lebensmittelfrage und revolutionäre Politik in Paris, 1792–1793: Studien zum Verhältnis von revolutionärer Bourgeoisie und Volksbewegung bei Herausbildung der Jakobinerdiktatur*. Foreword by ALBERT SOBOUL. (Ancien Régime, Aufklärung und Revolution, number 2.) Munich: R. Oldenbourg Verlag. 1979. Pp. 305. DM 58.

The “bread question” has not been ignored by historians of the French Revolution, and Susanne Petersen does not claim to have discovered it. Indeed, in the first pages the author makes very clear her allegiance to the “classical” interpretation developed by Jaurès, Mathiez, Lefebvre, and Soboul. Within this broad, interpretative framework, Petersen stakes out an important area for examination: how was the bread question connected to the formation of the alliance between the popular movement and the revolutionary bourgeoisie? Using an impressive variety of published and archival sources, the author concentrates on the critical months between September 1792 and June 1793 when the anti-Girondin coalition took shape.

Although ideologically close to Albert Soboul, who wrote an appreciative foreword to her book, Petersen cites and uses to good effect the most recent American and English work on bread supply and food riots in the eighteenth century. The results of this wide reading are apparent in the second, most general chapter on the problems administrators encountered trying to supply flour and bread to Paris during the early years of the Revolution. In-

cluded in this chapter are tables and maps that detail the number of bakers and levels of supply available in each section of Paris. Accompanying them is a sensible review of the major sources of controversy between the bakers and the various administrations in Paris. It is in this substantial chapter that Petersen makes her most original contribution.

In subsequent chapters, the author uses more conventional sources in her effort to unravel the political complexities of the bread question. She traces policy decisions and public debates at three levels: the Convention, the Paris Commune, and the sections of Paris. After close analysis of the different and evolving positions, Petersen draws three related conclusions: the Jacobin deputies only moved away from the Girondin position on free trade under pressure from the popular movement; the *sans-culottes*, on the other hand, did not originally tie the bread question to a demand for a maximum on grain prices; and, most important, the reconciliation of Jacobin deputies and the popular movement occurred only after the Jacobins in the Paris Commune had succeeded in politicizing the *sans-culottes*. In other words, the popular movement in Paris would not have had a national political impact if the radicals in city hall had not convinced the *sans-culottes* of the sections that bread could only be assured through the establishment of a new national policy.

The greatest weakness of this intelligent book is its abstraction. The Jacobins in the Paris Commune and the *sans-culottes* of the sections remain vague forces, vectors of explanation rather than actors. Petersen is so preoccupied with defending the progressive character of popular demands that she neglects the interesting possibilities presented in her own early chapters on food supply: in what areas and among what kinds of people did the Commune seek and find support, and why did this small group of men in the Paris city government have such an impact? Yet though it is at times heavy-handed, Petersen's work shows that German scholars continue to pursue a lively, wide-ranging interest in the most telling issues of the French Revolution.

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LOUIS BERGERON and GUY CHAUSSINAND-NOGARET. *Les "masses de granit": Cent mille notables du Premier Empire*. (Bibliothèque Générale de l'École des Hautes Études en Sciences Sociales.) Paris: Éditions de l'École des Hautes Études en Sciences Sociales; distributed by Librairie Touzot, Paris. 1979. Pp. 122.

Among the cooperative projects in quantitative history for which it is increasingly known, the *École des*

hautes études en sciences sociales has been sponsoring a study of the Napoleonic Empire's *notables*. A progress report was published in the September 1971 *Annales*, and now this slim but splendid volume offers some general results. The *notables* in question were the members of the permanent electoral colleges erected in each *département* and *arrondissement* of the empire; their ostensible function was to nominate individuals for posts in certain local and national governmental bodies. The institution constituted one of the social foundations of the empire and, for that matter, of the Bourbon regime that replaced it. The electoral colleges of *notables* in effect identified an enduring postrevolutionary elite.

The lists of *notables* are a gold mine for the quantitative historian. Most contain information on age, family status, profession at three points in time (as of 1789, during the Revolution, and as of 1810), and level of landed income. The present volume is preliminary in the sense that no statistical operations have been performed on the data but only simple tabulations or correlations of two variables. Meanwhile the authors invite other scholars to avail themselves of their machine-readable data, and anyone working on a geographic area of France or on a particular occupational group would be well advised to do so.

The two principal concerns in this elegant study are occupational structure and mobility and the distribution of wealth within various professions and geographic areas. The unique data in the lists make it possible to trace the impact of the Revolution on various careers from two vantage points. A profile can be constructed of the *notables* as of 1810, 80 percent of whom were adults in 1789. The task is then to show the various routes taken between those two points in time.

From the mass of clearly presented material a few conclusions stand out. The prerevolutionary magistrates and financial administrators, whose social and institutional worlds were shattered in 1789, adapted poorly; they tended to retire on their land rather than resume their old careers in new forms or enter new ones. In general the post-1789 regimes rejected the monarchy's *officiers* while holding out significant opportunities to its *commissaires*—specialists and low-echelon employees—as well as its lower-level military officers. The liberal professions seem to show long careers, adaptability, and shifts into the public sector. The data on wealth permit the authors to construct hierarchies of occupations and also to determine the relative distribution of income within specific occupations. Overall, the category of proprietors—taken in an extended sense to include those who lived off their landed income, well-to-do peasants, and (unpaid?) local officials—almost always constituted at least half of any given income level.

Of course this source has its limitations. Non-landed income is by definition excluded from the lists, and certain social types, such as active merchants who have not yet siphoned off their capital into land, do not appear. The awesome presence of the military is underrepresented (though not wholly excluded), and its problematic role under Napoleon and after is left obscure. Since former nobles cannot be readily identified on these lists, one misses the answer to an obvious question about France's new elites. Yet, for all that, the conclusions of this study are fundamental and compelling. Social standing was now based on "l'aisance que procure la terre." "But the electoral colleges clearly went beyond 'the plutocracy,' rooting the regime deeply in the more modest levels of property. . . . The principle was simultaneously revolutionary and sterile. It precluded the reconstitution of a society of orders, but it held back, by all the inertial force of inheritance, the dynamic of ascent by work and merit" (pp. 63-64).

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JOHN GRAHAM SMITH. *The Origins and Early Development of the Heavy Chemical Industry in France*. New York: Clarendon Press of Oxford University. 1979. Pp. xii, 369. \$98.00.

John Graham Smith's study recounts the first half-century of the chemical industry in France (with side glances at contemporary English developments), but the emphasis is on the troubled years of the Revolution and empire, when the principal lines were largely developed. The bulk of the book consists of three long chapters devoted to the manufacture of sulphuric acid, chlorine bleach, and soda ash, respectively. Scientific and business aspects are dealt with in detail along with more strictly technological developments. Organizing the material along product lines makes it difficult to follow all the intersectoral relationships, although it is hard to see how any other scheme would have avoided analogous problems. Each of the principal chapters is buttressed by some three hundred footnotes, almost all citing archival or other contemporary sources. A nonspecialist in the history of technology will easily accept the work as a definitive collection of the documentary evidence.

From the same generalist point of view, however, it must be said that the reader pays a high price for the harvest of facts. Smith announces his intention of writing for social and economic historians as well as for students of the history of science and technology. Yet he manifests what can only be called a marked distaste for generalization or theory, indeed a dogged determination not to ask any sort of larger questions. One need only turn to the last chapter,

entitled "Conclusion," to get the point. It turns out to be devoted almost entirely to branches of chemical production that did not find their way into the text earlier!

One obvious issue is that of the interrelations between science and technology during the Industrial Revolution. Although the chemical industry in France may not be typical, its history should help lay to rest any remaining bromides about the independence of the two. Not only were distinguished scientists (Chaptal, Berthollet, Lavoisier) directly involved, but also scientific work was stimulated, in these formative years for chemistry, by the need to understand ineffective or balking industrial processes.

Smith's narrative offers (unprocessed) food for thought regarding other historical questions as well. Technological advance appears in its full, frustrating, and messy complexity, with numerous failures, false starts, and promising beginnings destined to peter out or vegetate. Among other difficulties facing pioneers was the reluctance of industrial users to accept the "artificial" output of processes devised to overcome natural-resource scarcities. During the period, this applied to chlorine bleach and Leblanc soda ash but also to coke-smelted iron, and it held in forward-looking England as well as in France. How much was stodgy conservatism, how much true product inferiority? Or was there a systematic tendency for early paleotechnic processes to be less reliable than the even more empirical, but long-established, eotechnic ones?

Finally, the study sheds interesting light on the role of government policy in an age when the lessons of Adam Smith were making but slow headway against the combined forces of tradition and political crisis. Even in England, duties on raw-material imports, notably salt, were onerous. But the situation in France was worse, particularly when revolutionary zeal was whipped up by the exigencies of large-scale war. Terror, bureaucracy, and ignorance ran rampant, making it a wonder that the "skidding Revolution" survived its internal and external challenges. The embattled republic may not have needed scientists, as Lavoisier's judge is said to have told him, but it did need chemicals. Napoleon's empire proved institutionally more stable, but the demands of persistent war, economic as well as military, posed problems for the industry more often than they provided opportunities.

There were a few legislative bright spots. The salt tax was repealed for a time during the Revolution, before fiscal needs overcame the memory of the hated *gabelle*. Imperial legislation on dangerous and insalubrious industries, like much legislation and little else from the period, showed considerable foresight and even greater longevity.

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TED W. MARGADANT. *French Peasants in Revolt: The Insurrection of 1851*. Princeton: Princeton University Press. 1979. Pp. xxiv, 379. \$25.00.

In a brilliantly conceived, exhaustively researched, and highly readable volume, Ted W. Margadant has produced far and away the best study of the provincial response to Louis Napoleon's coup d'état of December 2, 1851. For a week following the dissolution of the National Assembly, a movement in defense of the republic exploded across rural and small-town France. This provincial upheaval has been regarded until recently as little more than a convenient excuse for the prince-president to unleash the final repression needed to establish the police state called the Second Empire. Margadant has shown that it was a great deal more than that.

Most critically, Margadant documents the politicization of important segments of the French peasantry that occurred during the Second Republic. Neither Marx's potatoes nor Eugen Weber's savages, the tens of thousands of peasants who mobilized to resist the coup and the many more who sympathized with them had undergone an important republican education since 1848. Within this framework, itself revisionist, the book successfully challenges several historiographical traditions.

Margadant correctly rejects the misery-caused-resistance thesis by clearly showing that rural artisans and a market-oriented middle peasantry were at the forefront of the movement. Their economic concerns derived from a crisis of abundance and were answered by a Montagnard platform of tax redistribution, tariff reform, and cheap credit. Republican ideas spread from towns to bourgs to villages following pre-existing networks of petty commerce and within pre-existing frameworks of male sociability—*chambrées*, *cafés*, *mutuelles*, factions, youth groups. With the growing repression, the central fact became the development of widespread, but relatively local, Montagnard secret society structures within these contexts. The organizational foundation for collective action was in place.

When they were forced to fight, the peasants and artisans responded, initially, as republicans defending themselves: "opposition to political oppression was the leitmotif of the insurgency of December 1851" (p. 223). Margadant thus effectively destroys the Bonapartist-conservative notion of a desperate *Jacquerie*. Thus, in the political awakening of the peasantry, as in so many other respects, the transitional character of the Second Republic is revealed. Despite the crushing impact of postinsurrectionary repression, the insurgent regions retained the Montagnard tradition and continued to respond to urban leftism, be it Radical, Socialist, or Communist.

Because of the remarkable depth of this study we are put into intimate contact with this new left of

peasants and artisans. Their voices and actions speak directly from the documents. Although rarely sophisticated, they showed a clear grasp of the political role they were playing. They are a far cry from the inert and grubby mass visualized by the contemporary bourgeois observers who provided the chief documentation for the pessimistic perspectives of Vidalenc and Weber.

Although Margadant comes closer to capturing the living reality of peasantry under the intertwining impact of industrial capitalism and the bureaucratic state than virtually any other writer to date, he confuses more than he clarifies by the use of the value-laden and ahistorical language of "modernization" theory. Is regionalism, even localism necessarily "anachronistic"? Were rebels in Bédarieux seeking traditional "vengeance" against the *gendarmes* they massacred, or were they fighting a revolution? How would their "excesses" have been regarded had the insurrection succeeded? Once we admit modernization terminology into the arena, we are virtually obliged to make negative value judgments about that which is not classified as "modern." The dualism of tradition and modernity also tends to obscure the causal patterns of social change. Modernization is not something that just happens. And for all the extraordinarily fine and nuanced analysis one finds in this book, when one asks the question "why were these peasants politicized?" the answer is not clear. Modernization theory is inherently taxonomic and tends to subvert the business of history: to grapple with dynamics of causation.

CHRISTOPHER H. JOHNSON
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KÄTHE PANICK. *La Race Latine: Politischer Romanismus im Frankreich des 19. Jahrhunderts*. (Pariser Historische Studien, number 15.) Bonn: Ludwig Röhrscheid Verlag. 1978. Pp. 284. DM 78.

This work is designed to be a contribution to the history of the "pan" movements. The subjects are "political *Romanismus*," which Käthe Panick defines as the politicization of the feeling of unity between France and its Latin neighbors, and the practical consequences of this ideology on French domestic and foreign policy. The notion of different European folk families, of which the Romance, German, and Slav were most important, won widespread acceptance in the nineteenth century, and pan-Slav and pan-German ideologies have at times influenced political thought and national policy in both Russia and Germany. Panick declares that the pan-Slav, pan-German, and pan-Latin movements were definitely related and interdependent, but she recognizes that they are of different weight and signifi-

cance and so have attracted different levels of interest and attention. This is the first monograph on France and the pan-Latin movement. Although it is true that the pan-Slav and pan-German movements developed out of the conception of three distinctive European families, there was no pan-Latin movement in the sense that there was a pan-Slav and a pan-German movement, and this monograph does not provide the kind of evidence that would justify its title.

The work is divided into two parts. In the first Panick assiduously pursues references to "la race latine" and "les nations romanes" in the works of French writers and publicists before 1850 in an effort to describe the origins and development of "Latin ideology." According to the author's analysis, this ideology was politicized by 1850, and the second part of the work searches for its practical effects on French policy in connection with Rumania, Italy, and Mexico. Panick's conclusions are indeed properly modest: all in all pan-Latinism did not really have much demonstrable influence on Second Empire events. We are asked to consider the Mexican venture a possible exception. In spite of the title's reference to the nineteenth century, the work concludes with a discussion of the place of Latin ideology in the debate over the 1870-71 debacle.

Pan-Latinism did not become a coherent ideology in France, it was not widely dispersed, nor were its disciples prominent or influential members of any powerful elite. Karl Epting (*Das französische Sendungsbewusstsein im 19. und 20. Jahrhundert* [1952]) dismissed the problem of modern Latin ideology as not very fruitful. Panick does not make it more important by her Procrustean procedure of including almost every French writer of the first half of the nineteenth century who happened to identify or to recognize cultural differences between peoples as a source or as a representative of an ideology similar in nature or in program to either pan-Slavism or pan-Germanism.

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HARRY W. PAUL. *The Edge of Contingency: French Catholic Reaction to Scientific Change from Darwin to Duhem*. Gainesville: University Presses of Florida. 1979. Pp. 213. \$15.00.

Harry W. Paul has written an interesting and highly useful book that will be welcomed by scholars of Catholicism, French intellectual history, and the history of science. His volume goes a long way in opening up the study of the impact of Darwinian ideas and scientific naturalism in France. Most of

the existing studies of this problem emphasize the positivist standpoint. Through extensive research in the printed sources, Paul examines the general intellectual milieu of Catholic scientific thought into which Darwin's ideas were injected. Although the thinking and concepts of the various writers are well delineated, the discussion might have been strengthened by a more systematic portrayal of the institutional structure of late nineteenth-century French science. The absence of fuller information on this matter is all the more surprising since Paul adopts a largely Kuhnian framework of explanation in regard to the reception of Darwin's ideas.

Three very important points emerge from the consideration of Darwin's thought among French Catholics. First, the French scientific community itself was honestly divided over Darwin, and opposition to his theories was not necessarily a reactionary intellectual stance. As Paul argues, many of the most respected French zoologists and botanists had severe reservations about evolution by natural selection. Second, the acceptance and rejection of Darwin related only in part to the scientific validity of his ideas. Anticlerical scientists often embraced Darwin because of the antitheological implications of his thought. Catholic scientists often objected to his theories for just the same reason. Again, the reader would like to know more about the politics of the French scientific community. Finally, the author delineates several distinct intellectual traditions within French science and, more particularly, French biology. He makes it clear that there were Catholic scientists and Catholic priests who accepted Darwinian analysis. In this process, Paul convincingly argues that Teilhard de Chardin had numerous French predecessors. The examination of these traditions suggests that the concepts of so-called national styles of science may indeed have validity.

These two chapters on the French reception of Darwin should enjoy a wide audience and will aid understanding of the comparative treatment of evolution by natural selection. The remainder of Paul's volume is somewhat less successful. There are two extended intellectual portraits of Albert de Laparent and Pierre Duhem, who wrote extensively on matters of science and religion. Their books and articles illustrate two courses of scientific apologetics for religion. Both scientists sought to prove that religion was not at odds with science. De Laparent stressed that science ultimately proved that the physical world was a realm of order if not necessarily a realm of mechanistic order. Duhem was much more sophisticated and original. He contended that scientific theories are not explanations of nature but rather mathematical descriptions of physical laws based on empirical observation. By their very nature such laws could not be used to address religious

and metaphysical questions. Duhem sought to make physics an intellectual discipline wholly autonomous from religion. This outlook aroused criticism from both believers and anticlericals.

The discussions of De Lapparent and Duhem are difficult to follow, as are the implications of their thought for French science and philosophy. Nonetheless, these chapters do illustrate the complexity of the interrelationships of science and religion at the turn of the century and also the considerable intermeshing that lingered. In this regard, Paul's book makes a significant contribution and should be read alongside James R. Moore's recent *Post-Darwinian Controversies*.

Finally, this book stands as a sound corrective to the generally progressionist bias of much intellectual history that has concentrated on Protestantism and enlightened secularism. One of the pressing needs for intellectual history is a more careful examination of modern Catholicism, and Paul has made an excellent contribution to that now-obscure area of research.

FRANK M. TURNER
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JACK J. ROTH. *The Cult of Violence: Sorel and the Sorelians*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xi, 359. \$25.00.

Since the early 1950s there has been a consistent scholarly interest in the work and influence of the maverick French intellectual Georges Sorel. He is chiefly known for his *Reflections on Violence* (1908), where he exulted in the violence of turn-of-the-century syndicalism, anticipating the redemptive effects of a resurgent class struggle on the decadent moral qualities of modern life. But Sorel served in many capacities during his life other than as the theorist of the revolutionary general strike. Prior to 1906–08 he had been an advocate of the evolutionary Marxist revisionism of Eduard Bernstein and a prominent Dreyfusard during the years of the affair in France. After his revolutionary syndicalist phase he flirted with Maurrasian monarchism and nationalism, with bolshevism after 1917, and, during his last years, with Mussolini's Young Fascist movement. The main "problem" of Sorel scholarship has been to trace Sorel's path through these astonishing changes in political course and, if possible, to explain them.

Jack J. Roth, whose earlier papers and articles helped stimulate contemporary interest in Sorel, has provided in this book a definitive map for tracing the course of Sorel's political odyssey. The book is divided into sections corresponding to the major political episodes of Sorel's career, and each section is broken down further into chapters entitled "The

Man," "The Idea," and "The Impact." A final section on "Sorelians after Sorel" traces the influence of his thought through the interwar period. In spite of the schematic appearance of this organization, the book follows a coherent chronological path throughout.

Roth's stated aim is to study the evolution of Sorel's "apocalyptic politics." He does this masterfully and in exhaustive fashion, tracking down even the subtlest variations in Sorel's ideas in all the obscure French and Italian periodicals for which he wrote. This would in itself have been adequate service to scholarship, but Roth also samples the reactions of Sorel's contemporaries to his work and attempts to assess his influence on his chief political disciples: Paul Delesalle, Edouard Berth, Georges Valois, Arturo Labriola, Giuseppe Prezzolini, and a host of others who are less well known. By viewing each of Sorel's changes of course in its comprehensive political and ideological setting, Roth is able to explain many of them as sensible responses to variations in the political climate or as the old man's practical effort to find a suitable vehicle for a moral *ricorso*.

Occasionally the search for influences within the syndicalist or nationalist grassroots becomes a bit mechanical; moreover, the reader looking for new insights into Sorel's more formal thinking will not find them here. Roth makes only token efforts to keep the reader abreast of Sorel's quite remarkable grasp of the main scientific and philosophical movements of his day. There are many indications that a full-scale intellectual study of this fascinating figure that follows his interests in Bergson, pragmatism, the philosophy of science, and the natural and social sciences will reveal the reasons and basis for his search for a revolutionary renewal. Such a study will also shed light on the nature of the intellectual synthesis that convinced Sorel and many of his contemporaries that they had decisively narrowed the gap between revolutionary theory and practice.

ROBERT A. NYE
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PAULA HYMAN. *From Dreyfus to Vichy: The Remaking of French Jewry, 1906–1939*. New York: Columbia University Press. 1979. Pp. xii, 338. \$17.50.

Since the Second World War, French Jewry has generally enjoyed a poor historical press. It lacked the strength and diversity of American Jewry, the great intellectual accomplishments of the Germans, and the strategic leverage that British Jews were sometimes able to exercise over Palestine; all the glamor and achievement seemed elsewhere. Then too, the intensity with which French Jews assimilated into French culture has, in retrospect, ap-

peared overly apologetic and politically unwise. Perhaps apprehensive of such criticism or lack of appreciation, a principal organ of the Jewish community in France, the *Consistoire central*, has adopted a more stringent policy regarding access to its archives than the government of Valéry Giscard d'Estaing.

Paula Hyman's stimulating book on the Jews in France from 1906 to 1939 stands with those who re-evaluate this view. Not that she is overly kind to the native French Jewish leadership. On the contrary, her material amply confirms the predominant view of an anemic and short-sighted policy in the face of mounting antisemitism in the 1930s. Rather, she helps to shift attention from this indictment to a less familiar theme—the emergence of “a broader conception of Jewish identity, which included the ethnic and cultural elements formerly discarded as the price of assimilation” (p. 233). Like David Weinberg, whose monograph on the Jews of Paris during the 1930s appeared in French in 1974, she devotes considerable time to immigrant Jews from Eastern Europe, whose arrival in France confronted established Jewry on many levels—institutional, cultural, social, and political. There is a good discussion of the vociferous Jewish labor movement that “challenged the quietistic political stance of the organized Jewish community” (p. 89). More important, she suggests that assimilation took on new forms in the period between the wars: although the immigrants' own institutions facilitated their integration into French society, elements of the established Jewish community began to move toward a new pluralism, a fresh symbiosis of Jewishness and Frenchness that pointed to “a tenuous biculturalism.” Youth groups, she suggests, were particularly creative in this respect, despite manifest opposition to such a notion on the part of both traditional Jewish leaders and the regnant ideology of French society that was (and largely remains) hostile to cultural difference.

To describe this complex and sometimes obscure process of change, Hyman has cast her research net extremely widely. Although unable to use the archives of the *Consistoire* for the period after 1925, she tracked down material in more than a dozen other archives in the U.S., France, and Israel. She rummaged extensively in the Yiddish press that flourished in France (no less than 133 Yiddish periodicals were launched between 1918 and 1939!), offering a cornucopia of political views, from Trotskyite to Bundist to Orthodox-religious. Her bibliography is exceptionally useful.

There are a few unfortunate slips. Great Britain did not close its doors to immigrants in 1905, and one should challenge the widespread contemporary idea that 1930s racism bore “a direct German influence” (p. 200). Conditions for refugees in France

were not alleviated in any important way in 1939, and certainly refugees owed no thanks to Foreign Minister Georges Bonnet, whose *Comité interconfessionnel* was largely window dressing to conceal a much more cruel strategy.

Finally, I wish that she had referred more often to Weinberg's study to compare notes and to help resolve some apparent discrepancies. For example, the two authors differ on the number of adherents to the *Consistoire central* (Weinberg cites 6,000 families as a maximum for the 1930s, while Hyman refers to 7,114 individuals) and on the last year in which the extreme right-wing *Croix de feu* participated in Jewish memorial services (Weinberg says 1936 and Hyman 1937). Although both agree on the uninspired strategy of the immigrants' *Fédération des sociétés juives* in the late 1930s, they clash as to causes: Weinberg stresses the viscerally apolitical cast of its refugee constituency—partly in reaction against the Bolshevik upheaval, from which many of them had suffered and partly due to the influence of religious Jews disgusted with Bundists and Communists—while Hyman seems closer to the populist argument of the labor movement, for which the *Fédération* was “composed primarily of middle-class immigrants” (p. 150) and was seduced by consistorial policies.

MICHAEL R. MARRUS
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JEAN-BAPTISTE DUROSELLE. *La décadence, 1932–1939*. (Collection “Politique Étrangère de la France.”) Paris: Imprimerie Nationale. 1979. Pp. 568.

Jean-Baptiste Duroselle's blunt and uncompromising title accurately reflects the severe tone of his book, whose theme is the traumatic record of French foreign policy during the 1930s. Much, of course, has already been written about the principal figures and the major episodes in the story, but this fact does not detract from the interest and importance of the book. For one thing, it is the work of a master historian, France's leading expert on modern diplomatic history; for another, Duroselle is the first historian to enjoy full access to the French Foreign Office archives for the period. The book bears considerable resemblance to Sir Llewellyn Woodward's *British Foreign Policy in the Second World War* (although Duroselle's judgments are more forthright and the tone is necessarily more somber). A successor volume dealing with part or all of the war years is promised by the author.

Following the tradition of his Sorbonne predecessor, Pierre Renouvin, Duroselle blends an assessment of individual statesmanship (or, more often, lack of it) with an analysis of the forces, interests, and attitudes that shaped or restricted the efforts of the policymakers. Midway through the book, the

chronological sequence is interrupted by probing chapters on "the ambiance," the links between economic interests and foreign policy, the condition of the armed forces, and the personnel and structure of the foreign policy establishment. But the central core is a year-by-year, at times day-by-day, account of how successive premiers and foreign ministers saw the developing crisis in Europe, what they did (or failed to do) about it, and why they acted thus. Although Duroselle expresses sympathy for the dilemmas faced by these men and recognizes that most of them had qualities of strength as well as weakness, he contends that the governing class as a whole must bear a heavy burden of "collective responsibility" for ultimate failure. "Men can, at certain moments, struggle successfully against deep and blind forces. But that was not to be France's destiny. In this instance, men proved weaker than fate."

Duroselle suggests that France's diplomats, for the most part, deserve higher marks than the politicians and Quai d'Orsay officials. "First-rate observers," they rarely failed to provide Paris with full and accurate information about events and probabilities in their respective capitals. This information, however, was not always wisely used. Liaison between the shapers of foreign policy and military policy was especially poor, and most foreign ministers of the thirties showed a scandalous "manque de sérieux." Georges Bonnet, Duroselle believes, was one of the few exceptions; he usually had a clear policy line in mind but zigzagged in typically opportunistic style and showed little concern for international morality or respect for commitments.

One recurrent theme in the book is the cavalier treatment of their French colleagues by British leaders. "The British method, up to and including 1939, remained the same: do not consult France in advance but take a decision and then communicate it to the French." Fundamentally, Duroselle believes, Franco-British tensions reflected a deep difference in general outlook (and perhaps even in social background of the two governing groups): the British reluctant to make commitments but feeling themselves honor bound to respect a promise once given; the French moved by "pactomania" but always seeking loopholes in any pact ("the Briand heritage," Duroselle calls it).

La décadence advances no startling new general thesis or ideological slant on French foreign policy in the thirties. Its strength lies in its careful, solidly documented, nuanced judgments about men and events; the author uses new archival evidence to reaffirm some things we knew (or thought we knew) and to shoot down some speculative, but dubious, theories that have been given currency by certain Soviet or British historians. Duroselle's book is one of the finest examples of a distinguished tradition in

French historical scholarship—a tradition that some regard as *démodée* but that is likely to endure longer than some of the more exotic products of history *à la mode*. And no reviewer can neglect a word of praise for its physical qualities; not for years have I seen a monograph so handsomely printed and bound. France's Imprimerie Nationale deserves an accolade.

GORDON WRIGHT
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VICENTE CÁRCEL ORTÍ. *Iglesia y revolución en España (1868-1874): Estudio histórico-jurídico desde la documentación Vaticana inédita*. (Colección Historia de la Iglesia, number 12.) Pamplona: Ediciones Universidad de Navarra. 1979. Pp. 682.

This is an objective, scholarly study of political and diplomatic negotiations between the Spanish Catholic Church and the Vatican during a critical period of change for both institutions. Not only did Spain undergo a revolution, but also the papacy lost the last of its territories to Italian armies and began immediately to formulate new policies for international relations at the First Vatican Council. Thus the traditional format of church-state relations was challenged not only in Madrid but also in Rome. Cárcel's organizing theme is Rome's success in avoiding either official recognition of Spain's interim governments or a break in diplomatic relations. This is the more solid of the two sections, the second being an interesting but piecemeal analysis of the clergy and of lay organizations in Spain during this period.

The book is tightly organized, although strict adherence to chronology fragments the excellent analysis. Cárcel deftly separates out the constituent parts of church-state relations. At the forefront is "religious liberty" as established by the Constitution of 1869, a complex political doctrine of limited pluralism; the state committed itself to financial support for the Catholic Church because the majority of the nation professed that religion but guaranteed equally the rights of Spanish Jews and Protestants. Cárcel deals extensively with the *patronato real*, a privilege long accorded by the papacy to Spain's kings, enabling them to control the appointment of bishops, but denied to the interim governments of this period. From various angles he studies the right of association that was constitutionally guaranteed for the first time in 1869. Although it was an affront to the tradition that only state and church had a right to organize Christian societies, it paradoxically enabled Catholic laymen to organize in defense of church interests. Finally Cárcel analyzes (more superficially) civil marriage, an assault not only on what the church considered a sacrament but also on its sociolegal control of the institution of the family.

This analysis, however, is not set within the context of Spanish society at large, much less of one undergoing major changes during "the Revolution." There is merely an affirmation of the continued religiosity of Spaniards that mirrors Cárcel's excessive reverence for a document, a tendency that characterizes the historical studies associated with the University of Navarre.

Undeniably better by far than traditional narratives that perpetuated factual errors or the bitter polemics that have so often obscured the issues, this method of history-as-document is perhaps more misguiding because more subtle. The sole fact that there exists a *written* version of an event does not endow that version with a supernatural dimension of truth. Modern scholarship requires not only that it be integrated into the total historic moment but also that there be a penetrating analysis of the personalities involved, as well as of the socioeconomic and intellectual "vested interests" (even though these may not be specifically mentioned in the document).

Cárcel indeed acknowledges the need for context by including a comprehensive annotated bibliography of socioeconomic and cultural, as well as political, studies on this revolutionary period. But they are not integrated into his discussion of two major discoveries: documentation showing that the pope consulted Spanish bishops about a policy rather than deciding alone or in collaboration with the state and that, for the first time, Spanish bishops openly opposed a papal instruction. He provides biographical information, for example on the bishop of Málaga, including the scandal associated with his administration; yet this is left to a footnote while, in the text, the bishop's lengthy descriptions of events are frequently cited. Events in Málaga were not that important on a national scale, nor were the bishop's declarations necessarily valid.

Historians will gratefully acknowledge this accurate, comprehensive presentation of documentation concerning a major issue, particularly material from the Secret Archive of the Vatican. But, insofar as this constitutes only "history from above," it distorts reality that, particularly in Iberia, is always complex and incongruent.

JOAN CONNELLY ULLMAN
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WERNER BUCHHOLZ. *Staat und Ständegesellschaft in Schweden zur Zeit des Überganges vom Absolutismus zum Ständeparlamentarismus, 1718-1720*. (Acta Universitatis Stockholmiensis, Stockholm Studies in History, number 27.) Stockholm: Almqvist and Wiksell. 1979. Pp. xii, 189. 62.50 KR.

Werner Buchholz's overriding concern is with the effective limits to the power of Sweden's absolutist

monarch and absolutist administration, respectively. Specifically, he has chosen to study the "position, influence, and significance of the *Beamtenchaft* and its relationship with the economy of the country" during the transition from Carolinian absolutism to the parliamentary sovereignty of the Age of Liberty.

After an introductory presentation of the historiography and of its own *problematique*, the book contains three major chapters. The first of these deals with the opposition of the administrative elite to Charles XII's ad hoc financial reforms of 1718 that, among other things, involved the withholding of all administrative salaries in excess of 200 *daler silvermynt* per year. The author's careful analysis of the financial records demonstrates for the first time that several provincial governors and other ranking officials, while showing their obedience to the royal commands in their correspondence, simply ignored the orders and continued to pay out salaries in full.

In chapter 2, Buchholz deals with the restoration of Charles XI's administrative system and the efforts of the administrative elite after Charles XII's death to protect their salaries from any future ad hoc innovations. Here, as in chapter 3, discussion of the ability of the administrative colleges to maximize their influence in the then-sovereign *riksdag* by placing two members of each college on each diet committee is a very important contribution to scholarship.

Finally, in chapter 3, Buchholz traces the unsuccessful attempts of the central administration to expand its jurisdiction and power at the expense of the church and the towns at the *riksdag* of 1720. Just as the first chapter demonstrates the limits to Charles XII's "absolute" power, chapter 3 reveals the limits to the power the central administration attempted to exercise after that monarch's death. The author's treatment of the topic clearly shows the continuity of the ambitions of the absolutist administrative apparatus despite the shift in sovereignty from the monarch to the estates.

This Marburg dissertation is well written, engaging, and thoroughly researched. It contributes not only to our knowledge of early eighteenth-century Sweden but also to the broader debate on the nature and limitations of the absolutist state. Only in a few instances does Buchholz slip and allow his theoretical framework—based, among other things, on the quite reasonable assumption of a community of interests between the administrative elites and certain social and economic segments of Swedish society—to serve as an at best tenuously documented argument.

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OLE FELDBÆK. *Denmark and the Armed Neutrality, 1800-1801: Small Power Policy in a World War*. Translated by JEAN LUNDSKAER-NIELSEN. (Københavns Universitet Institut for Økonomisk Historie, number 16.) Copenhagen: Akademisk Forlag. 1980. Pp. 305.

This is a definitive treatment of the role of Denmark in the League of Armed Neutrality of 1800 and an authoritative study of the role of the other nations involved in it. Ole Feldbæk has made extensive use of Danish manuscript sources and selective use of the manuscript sources of Sweden, France, Britain, and Prussia. In addition, he has consulted the Russian published sources and literature through a translator. Many of these sources have been used previously, but until now no one has examined so many of them together. What especially distinguishes this study is the author's use of the records of the Danish Council of State and the private papers of Christian Bernstorff and other Danish statesmen active in the council.

The outline of this story has been sketched many times, but Feldbæk has examined it closely and presents much that is new:

(1) The commercial policy of Denmark prior to and during the crisis was one of "offensive neutrality," that is, the Danes were trying to maximize the advantages that their commercial establishment anticipated from the broad interpretation of the rules of neutral trade. It was a decidedly daring policy.

(2) The Danes understood perfectly well their exposed position as the advance guard of the Armed Neutrality at the entrance to the Baltic, and they understood that Tsar Paul intended to use the Armed Neutrality as a weapon to get his way at Malta (the "militarization" of the League). Nevertheless, they persisted in the face of the likely loss of the battle of Copenhagen because the alternative was to offend Paul and thereby to risk his punishing Denmark by supporting the Swedish annexation of Norway.

(3) One of the fundamental reasons why the Swedes did not assist the Danes more effectively to prevent the entrance of the British fleet into the sound was that the Danes did nothing to encourage Swedish assistance at that point. They were convinced that Swedish assistance would lead to a revival of Swedish claims to a share of the sound dues.

(4) The Danish role in the battle of Copenhagen is more clearly explained here than in any common European language, including Dudley Pope's *The Great Gamble* (1972). When the battle was over, it was clear that the British were in a position to destroy at will both the Danish navy and the city of Copenhagen.

(5) After the battle, there was a popular revulsion in Denmark against the greed of the shipowners and the policy of the government that supported

them (this is an interesting point that perhaps deserves more development), and the policy of "offensive neutrality" was soon very much moderated.

Of course, the nation that played the tune to which the other members of the Armed Neutrality danced was Russia, and Feldbæk has devoted more attention to the admittedly confusing nuances of Russian policy in this period than have most historians. Still, he is not quite on target. He describes the policy of Tsar Paul in terms of a "grand design," that is, a plan to ally with Bonaparte and to partition the Ottoman Empire. But some of the evidence that he himself presents is not consonant with such a design.

Feldbæk was not well served by his translator or his editor, especially by the latter. The English ranges from the merely ordinary to the awkward and incorrect. Still, for all of us but the small minority who read Danish, it is fortunate to have the work in English. The book has a full bibliography and reference notes, commentary on both sources and literature, an index, and a summary in Danish.

HUGH RAGSDALE
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KEIJO VIRTANEN. *Settlement or Return: Finnish Emigrants, 1860-1930, in the International Overseas Return Migration Movement*. Translated by SIRKKA BATTARBEE and KEITH BATTARBEE. (Studia Historica, number 10.) Helsinki: Finnish Historical Society; distributed by Akateeminen Kirjakauppa, Helsinki. 1979. Pp. 275.

The subject of the returned migrant has been studied by only a few historians and for few countries (notably Sweden, Ireland, Italy, and Greece), but no study to date approaches that by Keijo Virtanen in depth and quality. By use of passport registers, parish records, records of returnees, letters of migrants, questionnaires, interviews, sampling in detail of six representative districts of Finland, and comparisons with districts previously surveyed, Virtanen has produced a virtually definitive study not only of the returnees but also of Finnish emigration generally. It and Reino Kero's thorough general study of Finnish emigration will provide historians with much of what is known about the subject.

The author begins with an introduction to his methodology and compares his with that of other historians of returned emigrants. In fact, he brings his methods into almost every page of the book. With innumerable statistical tables, charts, and maps, the author shows the complexity of the movement of persons and families in terms of class, economic status, religion, political persuasion, deaths, age groupings, transportation, and business cycles. In the second chapter, Virtanen seeks to determine the volume of emigration and return migration. He

finds that for his six sample districts the percentage of returnees was about 15.9 percent, but he also shows that in all of Finland percentages varied greatly. He later makes a rough estimate for all of Finland of a 20 percent returnee rate. He proceeds to show the cyclical variations of the returnee migration, with peaks in the 1890s, the early twentieth century, after World War I, and in the 1930s. The distribution among economic and social classes is examined, such as for crofters, seamen, and skilled workers in mines and the lumber industries. A chapter is devoted to demographic aspects: age, sex, and marital status. In chapter 6 Virtanen moves over to North America and shows the distribution of the Finnish emigrants by district of origin, and he indicates the reasons for a higher returnee rate from the eastern United States than from the middle west and western Canada. The experience of the Finns in Australia, and to a lesser extent in South America and Africa, is examined in this chapter as well. The problem of alienization in the first generation emigrants is discussed. A significant chapter deals with the reaction of the returnees to the Finnish environment, and another deals with the impact of the returnees (which was rather minimal except in some rural areas). An excellent summary organizes all of the detailed material of the preceding chapters.

There are statistical appendixes, sample questionnaires, and a long bibliography. The statistical appendixes include the fluctuation of Swedish and Finnish emigration and returned emigrants, 1862–69; distribution by different periods for the six sample areas; monthly fluctuations in Finnish return migration, 1892–1937; the number of men and women returning annually to certain sample areas; location of settlement abroad by emigrants from sample areas in 1873, 1882, 1890, and 1905; and the last place of employment of returning migrants. Regrettably there is no index. In general, the volume should serve as a model for similar studies of other nations.

CARLTON C. QAULEY
Minnesota Historical Society

CARL C. CHRISTENSEN. *Art and the Reformation in Germany*. Athens: Ohio University Press or Wayne State University Press, Detroit. 1979. Pp. 269. \$18.95.

Carl C. Christensen has written several interesting articles touching on the relation between art and the German Reformation. Six of these studies have now been made to form the basis of a book. Contrary to what the title implies, the content of the work is restricted to a few select topics.

The first chapter is a brief description of the cult of images in the late Middle Ages and the earliest

iconoclastic writings of Karlstadt and riots in Wittenberg. Christensen's treatment of the cult of images is traditional and one-sided in its negativism; he does not sufficiently consider here the importance of pictures in personal devotion, a use that bore great fruit during the sixteenth century (where he again ignores the importance of book illustrations [pp. 110 and 123]). Chapter 2 contains a description of Luther's writings specifically on images, which is tacked onto a reprint of Christensen's earlier article on Luther's theology and the uses of religious art (with some small emendations).

Chapter 3 includes separate discussions of iconoclasm in Nuremberg, Strasbourg, and Basel; on all of these Christensen has published similar and extensive pieces. These are effective descriptions of the historical situations. It is not clear, however, why these cities were chosen; one suspects that, as with the art works in the next chapter, it was due to their thorough treatment by previous scholars. A brief conclusion on the significance of iconoclasm in German lands contains a compilation of some of the explanations for this phenomenon. A sounder perspective would have been provided by some reference to the iconoclasm of the time in Flanders and Holland.

The fourth chapter treats selected examples of early Lutheran art, almost exclusively from the workshop of Lucas Cranach. This is a limited group of works that has been studied repeatedly and in more detail elsewhere. This chapter includes an inconclusive discussion of the famous *Law and Gospel* composition and a much reduced version of Christensen's earlier study of epitaph monuments. There are some new connections made between Luther's writings and the *Christ and the Adulteress* paintings and useful summaries about the comparative iconography of altarpieces and the overall evaluation of early Lutheran art.

Chapter 5 is the reprint of a previously published article on the connection between the Reformation and the "decline" of German art. Christensen's exclusive focus on highly selective groups of frequently reproduced paintings is bad scholarship. It is also very misleading with regard to the richness and variety of art works produced at the time, especially in the crucial media of prints and drawings, which he unaccountably dismisses (a criticism that also holds for the preceding chapter). Artists throughout history have suffered the kind of hardship found in sixteenth-century Germany, and no artistic decline has resulted; in fact, there is ample evidence that sixteenth-century artists throughout Europe accepted as necessary the reorientation and purification of their art. There is no acknowledgment here of the situation in Flanders and Holland where similar conditions were accompanied by a great flowering of art in the later sixteenth century

(Pieter Bruegel and his contemporaries). In general, Christensen seems unaware of the richness and complexity of art production in the sixteenth century in Northern Europe; the Reformation in both its use and abuse of art did much to reaffirm the power and potential of the visual image.

In an excursus Christensen discusses Dürer's famous *Four Holy Men* painting, which he had partially studied before. It is very difficult to say much new about a painting to which no fewer than fifteen important studies have been devoted in the last twenty years or so; Christensen does not in any case try to resolve the problems surrounding the more controversial aspects of this work.

In general, then, this work is a somewhat uncoordinated and fragmentary treatment of a vast and difficult subject. (Throughout the book Christensen consistently disclaims the intention of taking up the more complex and far-reaching problems associated with his subject; for example, pages 13, 34, 124, 138, 162, 195, 248.) It includes a great deal of summarizing and collating of other scholars' views, copious reprinting of the author's own earlier works, and a few new, but scattered, suggestions and digressions.

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JOHN G. GAGLIARDO. *Reich and Nation: The Holy Roman Empire as Idea and Reality, 1763-1806*. Bloomington: Indiana University Press. 1980. Pp. xiv, 366. \$25.00.

John G. Gagliardo defines his goal as an analysis of "the currents of opinion on which the German consciousness of the Empire was based in the later eighteenth century." He works toward this end by an examination of selected political pamphlets and public commentary that surrounded the major crises in late eighteenth-century German political life: the Austrian effort to annex Bavaria and the formation of the League of Princes; the imperial election of 1790; the outbreak of the French Revolution and the unsuccessful imperial war effort; and, finally, the Napoleonic victories and the French pressure to dissolve the Reich. Emphasis varied, of course, with the particular event, but in virtually every instance Gagliardo discovers a strong concern for the Holy Roman Empire quite different from the mindless dedication "to a German habit" that Klaus Epstein attributed to German publicists in his *Genesis of German Conservatism* (1966). In particular, Gagliardo finds that, by the second half of the century, German jurists and political theorists were no longer thinking of the Empire as a state whose efficiency must be compared with that of the other European polities. Instead, they viewed the Reich

as a *Rechtsordnung*, a political, social, and cultural value system that stood for the limitation and dispersal of power rather than for its acquisition and centralization. They wanted to preserve the Empire because it offered a unique alternative to the power state and because its balance between imperial and territorial levels of government precluded a use of force that could stifle the cultivation of the individualism so central to enlightened German ideals.

That a number of its champions regarded the Empire as an institutional rebuke to *raison d'état* and based their defense on those terms speaks certainly to the existence in late eighteenth-century Germany of an intellectual group that regarded individual self-perfection as the basic rationale for political life. The question arises, however, as to the size of that group and to its actual influence. It is not clear to me from this study just how representative the sample of public literature might be. By the same token, I am not always certain how best to assess its impact in terms of actual behavior in the historic events under analysis. It seems to me that Gagliardo's goal requires three areas of attention: first, the documentation of a German consciousness; second, an assessment of the contemporary political literature; and third, a delineation of the linkages between that literature and the larger consciousness that he has identified. While the analysis of the political literature is thoughtful and convincing, the definition of a German consciousness beyond that of the writings themselves remains vague, and the connection between these writings and such a consciousness never takes shape.

JAMES ALLEN VANN
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BUSO VON DER DOLLEN. *Die Koblenzer Neustadt: Planung und Ausführung einer Stadterweiterung des 18. Jahrhunderts*. (Städteforschung, Reihe A: Darstellungen, number 6.) Cologne: Böhlau Verlag. 1979. Pp. xxxvi, 284. DM 98.

Unless one's perspective is narrowed by some polemical concern, the history of a town is as various as the people who lived in it. Although this study of Koblenz is concerned only with a small part, a short period, and a specific aspect of that community, it too demonstrates how wide an appeal urban history can make. At the most immediate level, it will be of interest to all those who live in or have visited present-day Koblenz and wish to know more about how the *Residenzschloss* and its environs first came to be built. Drawing on the voluminous documentation left by the electoral building commission, Buso von der Dollen gives a month-by-month account of just how the new quarter was planned and built. Al-

though his concern for precision and detail does not always make for lightness of touch, the persevering reader will be rewarded, as the Elector's first commission is translated into plans and as the plans are translated into bricks, mortar, and stucco. This exercise of the imagination is assisted greatly by the profusion of diagrams, illustrations, and maps, which add a very welcome visual dimension.

The architectural and town-planning aspects, however, are only one part of the story. At a different—albeit closely related—level, this investigation has much of importance for the social historian of eighteenth-century towns. Von der Dollen is as much concerned to explain why the new town was established as to describe how it was actually constructed. Koblenz was the de facto capital of the Electorate of Trier, and the new *Residenzschloss* was its center. As his statistical tables plainly show, the dominant, if not the only, influence was the Archbishop-Elector Clemens Wenzeslaus, together with his courtiers, his officials, and their swarms of servants and service industries. The community's social, cultural, and, above all, economic conditions all bore the imprint of their all-pervasive presence. It was somehow fitting that the opera selected for the opening night of the new theater (commissioned and financed by the Elector, of course) should have been *The Abduction from the Seraglio*. Although it would be tempting to interpret this inspired choice as evidence of social criticism as well as good taste, there is no suggestion that the dominance of the privileged orders was resented, rather the contrary. Koblenz may have been a cage, but its bars were lavishly gilded. When the armies of the French Revolution broke it open, its inmates found that they could not survive in the world outside: with the court gone, the economy of the city collapsed, and the population fell sharply. As these remarks indicate, there is much of interest for the political historian too in this stimulating and scholarly monograph.

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KARL-GEORG FABER. *Deutsche Geschichte im 19. Jahrhundert: Restauration und Revolution; Von 1815 bis 1851*. (Handbuch der Deutschen Geschichte, number 3/1, part 2.) Wiesbaden: Akademische Verlagsgesellschaft Athenaion. 1979. Pp. 319. DM 74.

The Gothic lettering and fierce spread eagle that adorn the cover of this book give it a distinctly Wilhelminian appearance. Actually the series of which it is a part was launched in the 1930s, then discontinued, only to be newly organized after the war under the editorship of Leo Just. Since 1956, volumes of the new series have been appearing, though not

in chronological order. With this volume by Karl-Georg Faber one of the last remaining gaps in the series is filled in.

In a German historical *Handbuch* the reader looks for compact rendering of prevailing scholarly opinion on the treatment of events and for authoritative bibliographical information. Within these terms of reference the Faber volume achieves its purpose very well. Compared with the more widely known *Handbuch* that bears the name of Bruno Gebhardt, which is now in its ninth edition and available in pocket-sized paperback, Faber offers a much more extensive account of the years 1815–51 and a bibliography that, for the present at least, is more up to date. Specialists and serious students will find it a useful book to have on their shelves.

The German Confederation provides the territorial frame for Faber's "German history." The Austrian lands within the *Bund*, however, receive relatively meager attention. The medium-sized and smaller states, on the other hand, are not slighted. Indeed, the encyclopedic coverage of the various smaller states, though laudable in principle, weights the volume more heavily on the political and constitutional side than the author may have desired, for he is fully aware of the attention to social and economic matters that is expected in a modern work. As he interprets his period, the explosive elements in German society before 1848 were more the reflection of preindustrial circumstances than of an industrialization in its early phase; and the revolutions of 1848–49 are to be seen not so much as the beginning of a new era as the end of an epoch in which monarchical "administrative states" had consolidated themselves after 1815.

Faber's own orientation is toward western Germany. Among his previous books, a volume on the Rhineland in Vormärz, as mirrored in the journalism and other writings on public affairs, is the most substantial; one of the most valuable sections of this new book—the one dealing with the resurgence of German nationalism in the 1840s—derives largely from this earlier work. Faber notes that his volume appears almost exactly one hundred years after Heinrich von Treitschke's first volume and fifty years after Franz Schnabel's. Schnabel's four volumes are still, he acknowledges, "the best general account of the intellectual and cultural structure of the period" (p. 10). Faber's aims are of course more modest. In his coverage some gaps are to be noted. The most striking perhaps is the absence of any direct attention, either in the text or bibliography, to the role of the military in Vormärz. Nor is there any attempt to include cultural history in a broader synthesis. Music, art, and the natural sciences are left out almost altogether.

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HEINRICH BEST. *Interessenpolitik und nationale Integration, 1848/49: Handelspolitische Konflikte im frühindustriellen Deutschland*. (Kritische Studien zur Geschichtswissenschaft, number 37.) Göttingen: Vandenhoeck and Ruprecht. 1980. Pp. 433. DM 96.

A slightly revised doctoral dissertation, this is a difficult book to review briefly. It is divided into many sections, which provides for a somewhat choppy presentation; but it is a very important book in which the author makes use of excellent sources, of computer analysis of quantifiable data, and of various social science models.

The scope of the book is wider than the title suggests. Although the concentration and most of the computer analysis are on the years 1848–49 (pp. 121–279), the first part of the work (pp. 16–120) provides a perceptive treatment of the tariff controversy from 1815 to 1848. The first part, with its wide range of topics and many incisive observations, is particularly choppy. Its two most important themes are the role of interest groups, which has been neglected generally, and the part that the tariff controversy played in the liberal opposition to restoration governments. The computer analysis of petitions in the second, major part of the book confirms the regional distribution of free trading and protectionist interests: "In the northeastern German coastal regions a free trading zone, . . . in the west, southwest and in southern middle Germany the protectionist core area," with the rest of the German Confederation relatively indifferent (p. 197). The mobilization of economic interests was the work of pressure groups, which, freed from the restrictions of the restoration era, came to acquire professional, paid executives and effective networks and engaged in extensive lobbying in Frankfurt. The most important groups were the protectionist *Allgemeine deutsche Verein zum Schutze der vaterländischen Arbeit* (ADV), led overwhelmingly by mining, textile, and iron industrialists (table 13, p. 219; table 15, p. 235), and the free-trading *Deutscher Verein für Handelsfreiheit*. Overwhelming support went to the protectionist interest groups that managed to mobilize support from diverse social groups in their core area: practically all artisans, a large proportion of industrial workers, almost all wine growers and tobacco farmers, a substantial part of other landowners as well as merchants (table 4, p. 149).

Heinrich Best has provided the best treatment of the tariff controversy in the German Confederation from 1815 to 1849. In addition, he draws a number of important conclusions. (1) Using Stein Rokkan's paradigm, he maintains that the conflict between free traders and protectionists was determined by regional rather than functional factors and that regional differences were increased rather than diminished by the beginning of industrialization. He fur-

ther concludes: (2) "The traditional estimate that proceeds from the organizational weakness and regional division of the 'bourgeoisie' cannot be confirmed" (p. 288). For it was active in pursuing its interests on the national level and very capable in reconciling interest conflicts within its ranks and in drawing on the support of other social groups regionally. This is an invitation to re-examine the role of the German bourgeoisie in the first part of the nineteenth century. Best also asserts (3) that the mobilization of the workers and artisans on the part of the bourgeoisie did not show fear of the masses. The ADV fully accepted the political gains of March 1848 and explicitly supported universal manhood suffrage, the regulation of working conditions, and the introduction of some social security. It was thus not reactionary. Best suggests: "The assumption that here was expressed a genuine readiness of the industrial bourgeoisie to carry out (*mitzutragen*) fundamental political and social reforms should at least be considered" (p. 171). On the other hand it is of course possible that it was making irresponsible promises to gain workers' and artisans' support. Although admitting that some members of the industrial and commercial bourgeoisie expressed fear of revolution, Best claims that such fear was more common among the educated bourgeoisie (*Bildungsbürgertum*). While hesitating to generalize, he provides strong evidence that the standard view that a major reason for the failure of the German revolutions of 1848–49 was the reconciliation of the bourgeoisie with the forces of reaction because of fear of the revolutionary masses should be re-examined. Lastly, (4) Best's explanation for the failure of the German economic elites to seize political leadership in 1848–49 lies in their split into free-trading and protectionist camps. But did it matter that the Frankfurt Assembly was unable to agree on economic unification once Austria and Prussia blocked political unification? And was the bourgeoisie's pursuit of its economic interest a compensation for its lack of political initiative in not standing up to reaction?

Best has not provided a definitive rejection of established interpretations; he has, however, shown how quantitative analysis can lead to the questioning of orthodoxy and has called for further work on German history in the first part of the nineteenth century.

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KLAUS TENFELDE. *Sozialgeschichte der Bergarbeiterschaft an der Ruhr im 19. Jahrhundert*. (Schriftenreihe des Forschungsinstituts der Friedrich-Ebert-Stiftung, number 125.) Bonn-Bad Godesberg: Verlag Neue Gesellschaft. 1977. Pp. 738. DM 98.

Labor and the labor movement under the impact of industrialization rank high among the interests of modern German social historians. Within this field there are three distinct lines of research to be identified. The oldest is concerned with the history of ideas and organizations that formed the labor movement. The second, more recent line focuses on the social history of labor and deals with living conditions and ways of life as well as the changes undergone during the process of industrialization. The third and most recent line of research combines the other two and, at the same time, goes beyond them. It implies a notion of social history as the history of the material basis and mentality of the labor movements. Integrating the social history of labor and the labor movement, this new research is interested in the formation of modern industrial society. It functions as the threshold between the older and narrower conception of "social history" as an academic discipline and the new concept of a "history of society" embracing the full spectrum of economic, social, and political history. A distinct development can be identified: the tools of research have become more complex and more complicated, the area for empirical observation has been narrowed while the time frame has been extended. Regional studies of specific branches of industry have become popular: a certain group or class of workers within a clearly defined geographical area is observed over a long period of time so that continuity and change under the influence of industrialization can be observed in a long-term perspective.

Klaus Tenfelde's substantial dissertation is a pioneer work in this transition from a traditional concept of social history to an integral interpretation. Three features are prominent in this transition. (1) It involves an interdisciplinary approach, including such diverse disciplines as demography, economic history, legal history, industrial sociology, and ethnology, which Tenfelde uses as his basis. (2) This transition generally implies having a fresh look at the archival sources, as usually the respective source material has been evaluated under different considerations or not at all. This is especially true for Tenfelde, who worked his way through a large number of regional and local archives. (3) In order to build up an integral history of society from case studies of particular groups or specific regions, however, it is not enough simply to imply the broad framework of the conventional wisdom and add a piece of new knowledge here and there. A constant reassessment of the larger framework and the actual research is needed, in which Tenfelde's book is exemplary.

Tenfelde looks at the economic conditions and social situation in the mining industry of the Ruhr district during the nineteenth century. He examines mentalities and ways of life, social formation, and organization of collective interests. The first part of

the book tells the story of working conditions and social development in the mining industry under the direction of the Prussian state administration during the first half of the nineteenth century. In the second, the author looks at the changes brought about through the reform of the mining law and the "unleashing" of the mining industry during the second half of the century. He places much emphasis on the consequences for the ways of life and mentalities of the miners. Finally, the changes in social formation and organization of interests among the miners are presented in the third part of the book, beginning with the traditions of the older corporations and ending with the mass strike of 1889.

There is also a political side to all this: Tenfelde demonstrates that the liberal effort to "unleash" the mining industry, that is, to free it from the control of the state through reform of the mining law, was among the most important steps to be taken by the authorities after the shock waves of 1848-49 in order to secure the support of the industrial bourgeoisie. For the workers, however, the change to a liberal system meant even greater vulnerability and stimulated the growth of organizations to protect their interests.

With all its complexity, Tenfelde's book still makes good reading. The author, now turned historian, has a solid first-hand knowledge of mining and the miners because he was one of them for years.

KLAUS J. BADE
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CHRISTOPH FREIHERR VON MALTZAHN. *Heinrich Leo (1799-1878): Ein politisches Gelehrtenleben zwischen romantischem Konservatismus und Realpolitik.* (Schriftenreihe der Historischen Kommission bei der Bayerischen Akademie der Wissenschaften, number 17.) Göttingen: Vandenhoeck and Ruprecht. 1979. Pp. 347. DM 75.

An up-to-date biography of Heinrich Leo has long been desirable in nineteenth-century German historiography. A prolific historian, Leo in his own day was almost as well known as Ranke. He proved to be one of the most formidable, if vitriolic, publicists for the Prussian conservative cause and, next to Friedrich Julius Stahl, was possibly its most respectable intellectual spokesman. An adherent of conservative Lutheran neo-orthodoxy, he was at the same time a trenchant critic of the institutional deficiencies of Protestantism and also played a leading role in ultimately abortive efforts toward rapprochement between conservative elements of the Lutheran and Catholic churches in Germany.

Christoph Freiherr von Maltzahn's study is mostly successful in showing Leo's historical significance. His book is very well researched, having drawn on available unpublished letters in West

Germany as well as on a broad array of primary and secondary literature. It is comprehensive in its coverage of Leo's life; his scholarly works; his political, social, and religious ideas.

The deficiencies of the work are largely due to its dissertationlike organization, style, and approach. The author provides a somewhat compressed narrative of Leo's life, followed by a short summary of his major works, with the bulk of the text devoted to a thematic treatment of his thought. Because Leo's ideas were so intertwined with his publicistic and political activity, this treatment inevitably entails some disorganization in recounting events in his life. Generally the work would have been more readable had it been written according to a standard biographical format—as a continuous narrative of its subject's life, alternating with discussions of various aspects of his writings and ideas. Very distracting, too, is the author's saturation of the text with snippets of quotations from Leo's writings. Furthermore, he presumes a thorough familiarity with the conservative politics and Protestant orthodoxy that informed Leo's life and work. Yet, in the case of a multifaceted figure such as Leo, it is quite conceivable that many readers interested in specific aspects of his career and writings might not be acquainted fully with this broader context. In this light, the work would have benefited greatly from a general introduction or conclusion.

Despite these defects, more of organization and style than of substance or interpretation, the work remains a sound piece of scholarship and thus a useful addition to the literature on nineteenth-century Prussian conservatism.

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GEOFF ELEY. *Reshaping the German Right: Radical Nationalism and Political Change after Bismarck*. New Haven: Yale University Press. 1980. Pp. xii, 387. \$30.00.

The central feature of German politics after 1890 was, Geoff Eley argues, the emergence of self-mobilized popular forces, the petty bourgeoisie and a militant peasantry, into the political arena, where the traditional parties of the right were compelled to try to accommodate them. The focus of Eley's study is the failure of the National Liberal party, structured on the basis of a patrician *Honoratiorenpolitik*, to respond to this challenge of "mass politics." One principal result of this failure was the mobilization of sectors of these popular forces into radical nationalist *Verbände*, especially the Pan-German League and the Navy League, outside of the traditional party system, where they were led by men who had been excluded by the conventions of *Honoratiorenpolitik* from access to influence in the Na-

tional Liberal party. These nationalist organizations in turn became spearheads of the "new right" and articulated a populist attack on the whole system of party politics.

Limitations of space make it impossible to do justice to the wealth of provocative argument and insight in this book. Particularly valuable are Eley's observations on the place of the nationalist *Verbände* in pressure-group politics, on populism, on nationalist ideologies and the problem of hegemony, and on the reintegration, after 1911, of the "old right" of the established parties and the "new right" of the nationalist groups and other popular organizations such as the Agrarian League. This book, in sum, presents a stimulating and challenging hypothesis about the flux in Wilhelmine politics, and it will surely result in a new appreciation for the dynamics of this political system.

The book is no less stimulating for being fundamentally wrong. Eley argues that the essential impulse in the nationalist *Verbände* was the frustration of political outsiders, who were "denied political access by the ingrained etiquette of *Honoratiorenpolitik*" (p. 184). This conclusion, however, rests only on capsule biographies of a handful of national leaders, not on any systematic analysis of who the local leaders actually were. These turn out in fact to have been, in a very substantial number of cases, precisely the *Honoratioren*, the leading businessmen, "the gentlemen from the law courts and other state officials" (p. 32), who dominated local government and ran local National Liberal politics. As Anthony O'Donnell has recently shown in his dissertation (Princeton, 1974), the system of *Honoratiorenpolitik* was far more flexible and responsive to the new "mass politics" than Eley believes, and the national *Verbände* were thoroughly woven into the social and political fabric of this system.

Eley is also wrong in arguing that the history of these organizations demonstrates the inaccuracy of the interpretive model of "manipulated social imperialism." His presentation of this model is a caricature and does no justice to Wehler and other proponents of the model. They themselves certainly admit that some of the national *Verbände* were self-mobilized and extremely resistant to attempts to manipulate them. Even Eley seems to confess that the "old right" did *try* to manipulate them (p. 358), although he is careful to use words like "absorb," "neutralize," "moderate," and "integrate" to describe this process. It is ironic that the principal subject of the book is the German Navy League, the least self-mobilized of all the nationalist leagues—the product of massive bureaucratic encouragement and an organization that in 1908 was quite blatantly and quite successfully "manipulated" by the imperial government. Eley's strictures against "manipulated social imperialism" do serve to warn

against an inflexible and uncritical application of this concept. But his book serves far less to undermine the model than to recast it in the language of Gramsci and Poulantzas. This is in itself, however, a considerable feat, and it will make this book essential for students of German political history.

ROGER CHICKERING
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NORBERT SCHWARTE. *Schulpolitik und Pädagogik der deutschen Sozialdemokratie an der Wende vom 19. zum 20. Jahrhundert.* (Sozialwissenschaftliches Forum, number 8.) Cologne: Böhlau Verlag. 1980. Pp. xiv, 514.

This topic is an exceptionally important one in the history of German Social Democracy for two major reasons. First, the modern socialist workers' movement originated in efforts by workers to acquire education; various worker educational groups of the late 1850s and 1860s gradually became politicized and served as the basis for the first working-class parties—both Lassalle's ADAV and the VDAV of Bebel and Liebknecht. Second, from its very beginning, Social Democracy was strongly committed to a didactic press, and as it matured, ever greater emphasis was placed on generating working-class consciousness through education. Wilhelm Liebknecht's proclamation that "Wissen ist Macht—Macht ist Wissen!" was taken quite literally by many in the movement.

Despite the cumbersome baggage it carries as a revised *Inaugural-Dissertation*, Norbert Schwarte's study contributes significantly to the growing body of specialized literature on prewar German Social Democracy. He ambitiously focuses on three aspects of the topic: socialist educational theory, especially its Marxian roots; how this theory related to the educational activities of Social Democrats, both inside and outside the Reichstag; and the extent to which these activities contributed to formation of a specifically socialist subculture in the years before World War I.

It is on the last issue that Schwarte provides the most interesting material. By carefully reviewing the party's educational activities during the critical years from 1890 to 1914, he is able to show how in theory German socialists were increasingly influenced by neo-Kantian educational thought and by other left-liberal experts in the field. At the same time, however, the radical wing of the party came increasingly to realize the importance of promoting socialist consciousness among working-class youth through the family. The integrative tendencies of the former were at odds with the exclusivity of the latter, but as on so many other issues the left was unable to carry the party with it. Schwarte's analysis reinforces a growing conviction that German So-

cial Democracy did not constitute a genuine subculture within Wilhelminian society.

The weaknesses of this book are primarily of form rather than content. Like most dissertations, especially German ones, it is excessively self-conscious about the trappings of organization (numbering chapters, sections, and subsections). Combined with social-scientific jargon, this organizational fetishism obstructs both stylistic elegance and easy comprehension. Had the dissertation been reworked into a polished book, this would have been a model study; but even as it stands, it is useful and enlightening.

GARY P. STEENSON
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WOLFGANG HÜNSELER. *Das Deutsche Kaiserreich und die Irische Frage, 1900–1914.* (Europäische Hochschulschriften, Series 3, Geschichte und ihre Hilfswissenschaften, number 106.) Frankfurt a./M.: Peter Lang. 1978. Pp. 295.

In the few years before 1914 the age-old "Irish question" reached an acute stage; the Third Home Rule Bill, the Curragh incident, and the Ulster crisis occurred in close succession and caused many observers to believe that Great Britain was close to civil war and thus likely to be restricted in its European diplomacy. Wolfgang Hünsele's book considers a side aspect of this question, namely, how the German empire judged the Irish issue when developing its own policy toward Britain. Was it tempted, for example, to encourage Sinn Féin in order to weaken Britain's capacity to intervene in Europe or at least to distract the British government from continental diplomacy to domestic issues? British super patriots at the time claimed that there were secret links between the "enemy within" and the "enemy without"—an assertion that appeared not as ridiculous as it may now appear, since Irish nationalists like Casement openly urged Germany to intervene, and Pan-Germans like General Bernhardt were eager to respond. Curiously, Ulstermen on the verge of open revolution against Asquith's Home Rule policy also threatened to elicit German aid: Wilhelm II would now do for the Protestant cause what William III of Orange had done in the late seventeenth century.

In fact, Hünsele demonstrates that the Germans themselves paid little attention to the Irish question. Although the German press commented on this problem, opinions followed predictable lines: Catholic, Liberal, and SPD papers favored Home Rule; but Conservative and National Liberal papers, those usually most hostile to Britain, opposed it. Wilhelm II and his advisers tended toward the latter position, not of course out of Anglophilia but because the parallel Polish question determined overwhelmingly their attitude toward ethnic minorities.

In consequence, none of the German plans for operations against Britain envisaged the exploitation of Irish nationalism or use of Ireland as a base. It does seem likely, however, that following the Sarajevo assassination the German government felt reasonably sure of British noninvolvement in European affairs because of the Ulster crisis; but apart from that temporary, and perhaps fateful, miscalculation the kaiser and his ministers were innocent of the designs attributed to them by British Germanophobes and cold to the approaches made by the Irish nationalists.

Hünseler's work was recently accepted as a doctoral thesis, a fact that affected its structure and its scope. But it is a useful contribution that clears up one hitherto uncertain aspect of pre-1914 politics in a very competent manner.

PAUL M. KENNEDY
University of East Anglia

PETER H. MERKL. *The Making of a Stormtrooper*. Princeton: Princeton University Press. 1980. Pp. xix, 328. \$16.50.

This analysis of pre-1934 Nazi stormtroopers is based on the vitas of 337 SA members culled from the "Abel Collection" of 581 early Nazis about whom Peter H. Merkl had written his earlier, more voluminous book. In this sequel the author is using essentially the same method of analysis that is implicitly statistical even though many of the counting, measuring, and test procedures are not spelled out in full, for the benefit of the interested. As for his findings, the author argues once again that in attempting to assess motivation for profascist proclivities, at least in the Germany of the late twenties and early thirties, historians do not have to relegate themselves to socioeconomic factors. Merkl dismisses such a monocausal explanation in favor of a multiplicity of hypotheses and theories dealing with several variables, some only definable in psychological terms, as would be xenophobia, the anxieties of youth, or an innate tendency toward violence. As such, his monograph makes good reading and constitutes, in fact, the most important major study written on the stormtroopers thus far.

This time around, however, Merkl makes some allowances for the possibility of socioeconomic forces as additional motivational background. But in dealing with such specific issues as job security or salary, Merkl generally tends to miscomprehend entire occupational and thereby social groups in late Weimar Germany, presumably because of a lack of familiarity with the pertinent issues at hand. His assertion, for instance, that the civil servants' motivations were "purely political" and "hardly class-related" reflects an utter misjudgment of a service class that in late republican Germany had as much

to suffer under Chancellor Brüning's tight-fisted budget measures (and thus had at least one reason to welcome Hitler) as did some independent shopkeepers whose businesses went bankrupt. Farmers, whom Merkl also misportrays, were neither as underrepresented in the Nazi party before 1933 nor as likely to be beyond "economic class" considerations as the author happens to believe (both examples from page 151).

A great deal of the fuzziness in this area undoubtedly has to do with Merkl's decision to dismiss primary and secondary sources germane to these and related subjects, such as the edited memoirs of former SA Chief of Staff Otto Wagener. In fact, one of the main weaknesses of the book lies in Merkl's failure to support his verdicts with evidence other than that drawn from his scanty sample and occasional secondary references; no back-up archival material was used. Even in the more immediate field of the SA Merkl has elected to ignore three previously published articles, one by the Canadian Erich G. Reiche, a second by the West German Mathilde Jamin, and the third by the young British scholar Conan Fischer. Another reason for Merkl's difficulties of interpretation may be a certain insensitivity toward contemporary German terminology and jargon. Hence, in one case the author translates what in the original German must have been *Edelkommunist*, quite a common expression in the twenties, as "noble Communist," whereas in proper translation it should have read "armchair Communist" (p. 271). As in Merkl's first volume published by the same reputable press, there are annoying mistakes resulting from careless editing and printing. Names of authors and German towns are just as much misspelled as ordinary English words; Hitler's Christian name is said to have been "Adolph"; page references turn out to be faulty (compare pp. 118, 195-96, 316, 326). Altogether, the book seems to have followed too hastily in the path of its larger and considerably weightier precursor, although the brevity of this work as such has much to recommend it to the student of this fascinating phase in recent German history.

MICHAEL H. KATER
York University

JOHN L. HEINEMAN. *Hitler's First Foreign Minister: Constantin Freiherr von Neurath, Diplomat and Statesman*. Berkeley and Los Angeles: University of California Press. 1979. Pp. x, 359. \$28.50.

Neurath has not had much of a reputation. Jovial in company but indolent and irresolute in office, he was said to be interested mainly in advancing his own career. He accommodated himself to Hitler and lent his name to a policy of deception and aggression. His ministry was stifled and ignored as

Nazi paladins invaded the diplomatic preserve, and ultimately it fell to his archrival Ribbentrop.

John L. Heineman's biography revises this picture of Neurath—at least in part. Neurath emerges as an intensely private man, at home more in the fields and forests of his estate than in the world of society and politics. He lived by the code of the civil servant, bound to offer disinterested advice and carry out instructions. But even when he was personally troubled by what he saw or heard, he was too inhibited by temperament and training to ask probing questions or to seek remedy. As foreign minister, he pursued policies that would destroy the system fashioned at Versailles and Locarno and would restore Germany to its place as one of the great powers in world affairs. Hitler concurred in these policies, for reasons of his own, and, because he had no personal experience in foreign affairs and lacked qualified advisers in the party, Neurath retained considerable influence. He shaped policy, determined timing and tactics, and protected his ministry against the incursions of Nazi amateurs. His quiet diplomacy often prevented precipitate or irresponsible actions; in fact, in moments of crisis he was often the only one who kept his nerve. In 1936–37, when Hitler turned more openly to expansionism, Neurath's position changed. He found himself isolated and increasingly in opposition. But fearful of what would happen if he resigned, he clung to his post until early in 1938, accepting and covering measures of which he disapproved. His final service to the regime, as Reich protector of Bohemia and Moravia, 1939–41, was simply the final humiliation.

Heineman has reconstructed Neurath's career with care and sympathy, and he provides the necessary context to make that career intelligible. His analysis of policy and policy making between 1933 and 1936 is by far the best part of the book, although his failure to distinguish clearly between Neurath and Neurath's very able senior staff reduces the force of the argument (and his neglect of the recent literature is baffling—and regrettable). His evidence on the extent to which Nazi policy was influenced by men outside the party challenges the view, put forward by Gerhard Weinberg and Hans-Adolf Jacobsen, that Hitler initiated a diplomatic revolution in these years. Heineman is less successful when he tries to use Neurath's career to explain why basically decent and honorable men cast their lot with Nazism. He adds little that is new, in part perhaps because he too readily accepts Neurath's own interpretation and that of his relatives and friends. Neurath's statements as ambassador and foreign minister, quoted copiously, suggest deeper, more complex motivations.

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WOLFGANG MICHALKA. *Ribbentrop und die deutsche Welt-politik, 1933–1940: Aussenpolitische Konzeptionen und Entscheidungsprozesse im Dritten Reich*. (Veröffentlichungen des Historischen Instituts der Universität Mannheim, number 5.) Munich: Wilhelm Fink Verlag. 1980. Pp. 371. DM 78.

For years, historians have needed an updated and scholarly examination of Joachim von Ribbentrop, Hitler's unofficial advisor on foreign affairs between 1933 and 1938 and thereafter foreign minister. Unfortunately, this study is not the book to fill that void. A reworked version of a doctoral dissertation (1976), this monograph provides little new information and reveals few new sources but summarizes accurately the principal works of the past fifteen years. It correctly argues that foreign and domestic policies of the Third Reich were not the result of monolithic planning and decision making but of constant infighting and "Bureau Darwinism." Wolfgang Michalka, however, throws little new light on the role Ribbentrop played in this planning anarchy; instead, the author offers a repetitive and ultimately nonpersuasive reconstruction of Ribbentrop's approach to world politics.

Surprisingly, Michalka relies almost exclusively upon printed materials, citing only a few unpublished documents from German archives. He has examined the recently opened British records, but these are not very helpful in clarifying Ribbentrop's influence. The absence of contemporary documents from Ribbentrop and his circle of friends stands out, for even while cautioning the reader, Michalka has relied heavily, and at times exclusively, upon Ribbentrop's memoirs, written in prison while awaiting execution. Repeatedly, Ribbentrop's version is cited, with no supporting or dissenting evidence.

The author asserts that Ribbentrop arrived at his foreign policy independently of Hitler and differed from his Führer's programmatic and ideological approach on a number of important points. Although this thesis is plausible, Michalka never really proves it but simply asserts and illustrates the differences through a discussion of Ribbentrop's policy toward Great Britain (which is consistently called England), France, and future colonies in Africa.

The author seldom leaves the rarified realm of theory to trace Ribbentrop's specific contributions to events, nor does he document the relationships among Ribbentrop and the other advisors in the "anarchy in planning." If Ribbentrop had really offered an alternative program to Hitler's ideological goal of Lebensraum in the East, the time for implementation was certainly the halcyon period following the Munich agreement. If Michalka is correct, the decision to occupy rump Czechoslovakia must have involved much debate between Hitler and his foreign minister on this point, yet the book simply

ignores the whole question.

As existing evidence indicates, Ribbentrop's approach to world politics was undigested, amateurish, and linked to his desire to ingratiate himself with Hitler and thus gain a career in the Third Reich. Under these circumstances, one cannot write a thoroughly satisfying monograph by limiting the subject to Ribbentrop's theories of foreign policy, for ultimately he was more an opportunist and manipulator. Ribbentrop's interest for historians lies not in his impractical theories but rather in his successful struggle for power, little evidence of which is to be found in this book.

JOHN L. HEINEMAN
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DEAN SCOTT MCMURRY. *Deutschland und die Sowjetunion, 1933-1936: Ideologie, Machtpolitik und Wirtschaftsbeziehungen*. (Dissertationen zur Neueren Geschichte, number 6.) Cologne: Böhlau Verlag. 1979. Pp. vii, 502. DM 80.

The subtitle clearly states the thesis of this dissertation. It is supported by a plethora of evidence culled from the German archives and the standard published collections of diplomatic documents. Dean Scott McMurry supports the proposition that Hitler's basic ideological commitment, from which he never strayed, was the destruction of the Jews-Bolsheviks. The ideological framework provided the means by which he intended to achieve the goals of power politics: the conquest of Russian lands and the enslavement of its peoples. Opposition from the Foreign Office establishment soon faltered, and it became subservient to Hitler's designs. In many areas of policy the diplomats' own ideas coincided with those of Hitler, especially in establishing Germany as the dominant European power.

McMurry perceives similarities between Hitler and Stalin. Both were motivated by ideology, both had designs for world power. The German was determined to achieve his racial and territorial goals at any cost. The Georgian did not let ideology adversely affect reality. Stalin could wait; Hitler, haunted by the possibility of his early death, could not. Stalin certainly did not consider himself immortal, nevertheless he appears to have been a leader who knew when patience was a virtue.

The author deals with every detail of German-Soviet relations during 1933-36, making it impossible to discuss every point made. He uses a strict chronological approach, except in the area of economic relations. Although quite a few new details are added to our knowledge, the greater part of what he has to say has already been published. An example will illustrate this criticism. For Stalin the Franco-Soviet pact was one more procedure to stabilize the West and, by so doing, also to create sta-

bility in the East. Hitler's refusal to be drawn into an Eastern pact, even without guarantees, disappointed the Soviet government but did not discourage it from attempting to improve relations with Germany.

In 1936, when German-Soviet relations had reached their nadir, with mutual vituperations filling the pages of the controlled newspapers and spewing like untreated sewage into the air, Stalin attempted to achieve political rapprochement. One avenue open was through economic negotiations. Germany needed Soviet raw materials; the Soviet Union needed precision instruments and modern technology (especially military), and economic relations continued even during the most tenuous times.

McMurry succeeds, admirably, in analyzing, condensing, and placing in context masses of material into a very comprehensive history of the first three years of Hitler-Stalin relations. The dissertation is a handy compendium facilitating research in that period. Historians should find it a valuable guide.

KURT ROSENBAUM
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ILSE UNGER. *Die Bayernpartei: Geschichte und Struktur, 1945-1957*. (Studien zur Zeitgeschichte, number 16.) Stuttgart: Deutsche Verlags-Anstalt. 1979. Pp. 297. DM 38.

In the literature on European politics since 1945, there is a dearth of research on minor, regional parties. Ilse Unger's excellent study of the history and structure of the Bavarian party (BP) is a major contribution toward filling this lacuna. Based largely on interviews and documentary sources, which the author uses skillfully but without exaggeration, the work offers a comprehensive analysis of the platform, tactics, and social bases of a party whose record is a study in consensus and cleavage, the former because of the appeal to all Bavarians and the latter because of its commitment to separatism.

The author defines the BP as a loose electoral alliance of particularist and monarchist groups, which had their roots in the Weimar era. In 1945-46, an effort to unite these factions into a single party foundered initially on the refusal of General Lucius Clay, the American military governor, to sanction a movement he believed to be reactionary and an obstacle to the reconstitution of a federalized Germany. The decision to withhold a license cast a shadow over the image of a party dedicated to the ideal of an autonomous Bavarian polity.

The skepticism of military government, the manipulative tactics of the two major parties, and ultimately the alienation of the church are the elements that Unger masterfully interrelates to explain the progressive loss of profile by the BP. Finally licensed in 1946 as the "Bavarian Democratic Union," the

BP was required to maintain a web of local organizations as a gesture toward intraparty democracy. The result was a factionalization of the movement to the extent that a consensual program eluded formulation. In the communal elections of 1948, the BP suffered from the conflicting ambitions of its leaders and gained the reputation of being a rejectionist party incapable of governing. Throughout successive coalitions with the Social Democratic party and then the Christian Social Union, the BP consistently found itself outmaneuvered. In this process Unger carefully describes the role of the clergy, which withdrew its support from the party in order to avert a splintering of clericalist forces. In the end the BP could only cling to its belief in a united Europe arising through a simultaneous devolution of governmental authority within nation states—a remarkably prescient position considering the resurgence of European federalism.

Unger's account of the formative period of the BP offers valuable insights into the founding of the Federal Republic of Germany. It is regrettable, however, that she did not fully develop the pattern of action and reaction triggered by the effort of military government to transplant American political values to Bavaria. An interpretation of the BP as a victim, in part, of a cultural confrontation would have enhanced the importance of this study for American readers.

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CARL E. SCHORSKE. *Fin-de-Siècle Vienna: Politics and Culture*. New York: Alfred A. Knopf. 1980. Pp. xxx, 378. \$15.95.

In seven essays—some of them published before and influential for years—Carl E. Schorske traces, in widely differing areas and media, the political and cultural responses to the rule and failure of Austrian liberalism from the 1860s to the beginnings of the twentieth century. The areas are architecture and urban development; the political style of the new mass movements; the political dimension in Freud's *Interpretation of Dreams*; instinctual revolt, its defeat and sublimation in Austrian art nouveau (Secession) and particularly in the art of Gustav Klimt; and the transformation of the garden image from mid-century *Biedermeier* via turn-of-the-century estheticism in Andrian and Hofmannsthal to the expressionist revolt of Kokoschka and Schönberg. (It is a pity that Schorske omitted the poet Georg Trakl, who would have supplied an excellent third to form a triptych of Austrian expressionism.) The common thread connecting these essays is the revolutionary consequence, for better and for worse, that the failure of Austrian liberalism spelled out for twentieth-century man.

Schorske points out that, unlike its British and

French counterparts, the bourgeoisie in Austria had failed to gain power by its own efforts. It was the beneficiary of monarchical absolutism's military defeats in 1859 and 1866. In Austria, the bourgeoisie succeeded neither, as in France, in supplanting the old aristocracy as the sole ruling class nor, as in England, in coalescing with it and remaking it in its own image. It had to content itself with remaining the rival of the aristocracy and continuing to seek recognition as an equal. The best road to this goal was imitation of the sumptuously aesthetic life style of the Austrian nobility. Intense appreciation and cultivation of the arts and striving for intellectual excellence were a consequence of the inferiority feelings of the Viennese bourgeoisie. Culture came to serve as a substitute for real power and true representation. The aesthetic and aristocratic way of life, which had been a status symbol for the fathers, became second nature in the sons and first nature in the grandsons. Riveting its gaze upon the class above, the Austrian bourgeoisie lost sight of the classes below itself.

Unfortunately, Schorske fails to clarify the fundamental difference in denotation and connotation that the term "liberalism" has acquired in contemporary America and that stands in sharp contrast to the meaning it had in nineteenth-century Austria. The contemporary reader is apt to associate with the term liberalism individual freedom, social equality, and economic justice; the liberal, as he understands him, believes in the duty of the state to bring about these goals. For the nineteenth-century Austrian, by contrast, liberalism meant political and economic individualism, which subsequently turned into a cult of inner sensibility to the exclusion of social concern. It left concern for social justice to the renegades from its own ranks who turned into its enemies.

Individual freedom, too, had its severe restrictions in the Austrian liberal ideal of man. This ideal, derived from Voltairian Enlightenment, German classical humanism, English Manchesterdom, and the early capitalist work ethic, was that of man as a purely rational creature, who was obligated to curb and repress his instinctual life. Political rejection of liberalism in the 1880s and 1890s was thus wedded to, or at least paralleled by, an oedipal revolt. Schorske shows very convincingly how the young former liberals, who founded the mass movements that extolled extraparlimentary appeal to the masses, nationalist and antisemitic ideology, and obstructionism and violence, had their counterparts in those pioneers in art and science who helped to revolutionize the modern world. For all their profound divergences, they faced the same problem: repression of the instincts and neglect of the social aspect of man.

The great strength of Schorske's collection of essays lies in the thorough and provocative analyses of

specific responses to liberal failure. His method is to focus on a limited topic, such as the reactions to the *Ringstrasse* in architectural philosophy; the psychobiographies of the antisemitic leaders, Schönerer and Lueger, and of the founder of Zionism, Herzl; the displacement of faith in political action by belief in science and preoccupation with the psyche, revealed in Freud's self-analysis of his dreams; the controversy over Klimt's paintings for the University of Vienna and its consequences for the transformation of a liberating artistic revolt into stylized decorativeness; and the sociopolitical significance of the transformation of the theme of the garden from Stifter's *Bildungsroman* to Schönberg's atonality. Schorske is peculiarly adept at identifying the problem and significance contained in a restricted topic. He follows the ramifications and interdependencies of politics and culture implied in his topics to amazingly revealing lengths. Pinpointing allows Schorske to plumb far greater depths and achieve much richer suggestiveness than more comprehensive studies could hope to attain.

Schorske's book is not, like William Johnston's *The Austrian Mind*, a comprehensive exposition of the whole rich field of *fin-de-siècle* Austrian culture; nor is it, like Janick and Toulmin's *Wittgenstein's Vienna*, an explicit analysis of the reasons for the peculiar relevance that *fin-de-siècle* Vienna has acquired for our own time and place. However, as the reader puts down Schorske's volume, he is compelled to think far beyond its explicit content. Therein lies the unique merit of Schorske's studies among several important works on Austrian culture that have appeared in the last decade.

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VINCENT J. KNAPP. *Austrian Social Democracy, 1889-1914*. Washington: University Press of America. 1980. Pp. x, 296. Cloth \$18.50, paper \$10.25.

The history of Austrian socialism during the interwar years has received far more attention, particularly from American and English scholars, than that of the era prior to 1914. This is understandable in view of the dramatic and ultimately tragic role played by the socialists under the republic, yet it is unfortunate inasmuch as the character of the movement was in large measure determined by its experience under the Habsburg regime. Vincent J. Knapp has endeavored to fill this gap. Basing his work largely on party and union protocols, published correspondence, and extensive reading in the most important German-language party periodicals and relevant secondary literature, he has produced a relatively straightforward account of the origins and ultimate disintegration of the Austrian Social Democratic Party.

Throughout his book Knapp stresses the moder-

ation of the Austrian movement and its lack of concern for ideology, both of which he traces to the leadership provided by Victor Adler. He is led to this interpretation at least in part by his very narrow view of ideology and by his tendency to oversimplify Marxist theory in general and the various Marxist concepts of revolution in particular. One result of this approach is that he finds it difficult to explain satisfactorily why the Austrian party produced, in Austro-Marxism, one of the most sophisticated and influential schools of Marxist thought within the Second International. Another is that he treats this group in a rather cursory and superficial manner. Rudolf Hilferding, one of its most original early protagonists, for instance, is not even mentioned.

In keeping with his central thesis, Knapp also pays little attention to the considerable emphasis placed upon the socialist *Endziel* in the party's popular literature or to the extensive educational apparatus developed by the party, which consistently endeavored to propagate an interest in the ultimate achievement of socialism. Finally, Knapp's stress on the gradualism of the Social Democrats and their ties to the existing state leads him to underestimate the degree to which they remained, in keeping with the Marxist doctrine of class struggle, intentionally isolated from potential political allies. He also ignores the socialists' tendency to develop, like their German comrades, a subculture within Austrian society.

Despite these limitations in scope and interpretation and an annoying number of stylistic and typographical errors, Knapp does provide a useful narrative that, one hopes, will encourage others to examine more carefully and fully this complex but important subject.

KENNETH R. CALKINS
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JEAN-CLAUDE HOCQUET. *Le sel et la fortune de Venise*. Volume 2, *Voiliers et commerce en Méditerranée, 1200-1650*. Lille: Presses Universitaires de Lille. 1979. Pp. 740.

This volume by a noted French scholar presents an important historical synthesis. Jean-Claude Hocquet's second volume concludes a massive study of salt and its relation to the economy of Venice in the Renaissance. In this volume he analyzes the development of Venetian shipping in round ships and how Venetian merchants after 1281 transported sea salt as ballast from outside the Adriatic. He relates how the Venetian state set the price of salt so high that the profits subsidized Venetian merchant shipping, helped finance Venice's wars, and enriched the Venetian patriciate. Consumers of this salt in the Veneto and Lombardy paid these high prices as long as Venice excluded competition. The peaks and depressions of this trade are analyzed and pre-

sented in graphic form as a result of the author's careful collection of data and study of metrology.

Within this larger framework, the author details certain developments. In his chapters on the Venetian round ships, he describes and distinguishes between the *nef*, *coque*, *taride*, *marano*, and *marciliane*. He charts their sizes and cargoes, their years of usefulness, and the rhythm of their voyages.

In analyzing the Salt Office, the author describes the *Ordo Salis*, in effect from 1281 to 1444, which obligated every Venetian who exported £100 Venetian in oriental goods from Venice by sea to bring back to Venice six measures of salt from beyond the Adriatic. The Salt Office served as middleman between these Venetian shippers and the Italian consumers of salt. Sums held by the Salt Office were drawn by the state to finance the land-based Venetian mercenaries and to subsidize the construction of great round ships. When the Salt Office could not reimburse the merchant shipowners in the mid-fifteenth century, a crisis ensued. Merchants transferred their credits to speculators, and the Salt Office began paying interest. Finally, after the return of peace to Venice around 1520, salt revenues increased. The Salt Office repaid its creditors and also amortized the public debt, which it had assumed.

Finally, Hocquet describes the end of this structured and profitable long-distance salt trade. When the Venetian merchant fleet suffered at the Battle of Lepanto and from competition with Ragusan, English, and Dutch ships, the importation of salt from outside the Adriatic declined. Smaller, less expensive round ships sailed for Venice. By 1600 the Venetians were again cultivating the nearby Adriatic salt marshes to supply their mainland provinces. The large foreign markets of Italy were finally lost to other suppliers—Genoa, Ragusa, and Ancona.

The data, the argument, and the conclusions indicate that these two volumes should be read as one work, although each volume has its own coherence. The bibliographies in both volumes are necessary to trace the footnote references in volume 2. Although the maps, tables, and graphs are integrated with the text, no index of them is supplied.

The magnitude of Hocquet's achievement cannot be underestimated. Hocquet's cautious generalizations concerning economic development are not derived from a model; he disavows any a priori concepts (p. 690). Instead, Hocquet bases his "provisionary results" on a posteriori reflections. He well deserved the Roberto Cessi Foundation prize, which he received for this economic and maritime history of Venice and the Mediterranean during four hundred significant years.

LOUISE BUENGER ROBERT
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SUSANNE SCHÜLLER-PIROLI. *Die Borgia Päpste Kalixt III. und Alexander VI.* Munich: R. Oldenbourg Verlag. 1979. Pp. 421. DM 38.

Seventeen years after her general history of the Borgias, Susanne Schüller-Piroli presents a detailed study of the two Borgia popes; a third volume on Francisco Borgia is yet to come. Her book makes pleasant reading, and it offers a balanced picture of these controversial men. She presumably intended to address only a lay, German audience, for she uses no footnotes and makes no claim to go beyond similar books in other languages.

This is a history of great men and women living in uneasy tandem with world-historical tasks. Calixtus III remains a shadowy figure, receiving a mere sixty-six pages of attention, apparently because he was dominated by, yet failed at, the task of effecting a crusade. The author allows no such collective task to dominate Rodrigo Borgia's actions, so his lifespan takes up fully five-sixths of this over-long book. Most of the book is taken up with retelling the diplomatic and military history of Europe during these years, with Rodrigo seemingly at the mercy of events, dedicated mostly to family affairs. Pope Alexander VI, for all the details, remains in the shadows quite as much as his uncle. He is a world-historical troublemaker.

Schüller-Piroli does properly insist that institutional morality had changed since the Renaissance, and she asks her readers to understand Alexander if not as a complex person at least as a single individual who saw no conflict between his private goals and those of the church. Her book often delights the eyes and imagination through its evocative descriptions and illustrations of present-day vestiges of Borgia buildings in Spain and Italy.

Much of the scholarly apparatus of this book was really unnecessary once the author had decided against footnotes. Yet the name index and the Borgia genealogical table will be important for the author's presumed audience. Those who enjoy the thickets of legitimate and illegitimate descent will have no end of pleasure with this book.

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GIUSEPPE LO GIUDICE. *Trieste, l'Austria ed il Canale di Suez.* Catania: Università degli Studi. 1979. Pp. 307.

Since we already have an abundance of books on the Suez Canal as well as works on Trieste's trade with the Near and the Far East, one may justifiably question if Giuseppe Lo Giudice's *Trieste, l'Austria ed il Canale di Suez* really represents a new contribution.

Any such uncertainties are dispersed by the author in his introduction where he explains that existing Italian works are, according to his experi-

ence, written in a polemical style or sometimes outright prejudicial toward the Austrian empire, especially if they were published during the Fascist period. This is also true for the question of the Suez Canal and Trieste's connections with it. To obtain an objective picture, the author therefore decided to review the unpublished and published archival materials located mainly in Vienna and Trieste but also found in Rome and Naples. The result of his painstaking research is the above book, and one has to congratulate Lo Giudice for his courageous destruction of the old nationalistic myths about the hostility of the Austrian government toward the Italian community in Trieste.

The book has four major parts. The first two describe Austrian involvement in the projected building of the Suez Canal. We learn that Austria played a much greater role than is usually recognized. Among the Austrian supporters of the canal were such personalities as Prince Metternich; the emperor's brother, Maximilian, the ill-fated emperor of Mexico; Karl L. von Bruck, the founder of the Austrian Lloyd in Trieste and later minister of economy and finances; and Luigi Negrelli, an Austrian subject from the Trent region, the technical expert on the canal and the founder of the "Société d'études pour le Canal de Suez" in Paris. When it came to the construction of the canal (1859–69), however, Austria could not participate because of the international difficulties associated with the wars of 1859 and 1866 and the domestic turmoil aroused by liberalization.

The third part deals with Trieste's economic crises on the eve of the opening of the Suez Canal, which were principally caused by the delayed and costly replacement of sail vessels by steamships and by the general need to modernize Trieste's obsolete commercial methods. The author ably depicts the great expectations of Trieste's commercial circles that the opening of the canal would substantially increase their Far Eastern trade and cure their economic ills. In the last part of the book the author elaborates on the causes of these shattered expectations. He emphasizes that Trieste's backward economy, and not the Austrian government, represented the true reason for the short-term failure. This was also confirmed by later events. After the modernization of Trieste's economy, trade with the Orient increased and became very profitable by the turn of the century. Furthermore, the abolition of the city's free-port status in 1891 enabled Trieste to manufacture final or semifinal products out of imported raw materials in its own factories or in the factories of the nearby hinterland.

The book is richly provided with statistical tables, graphs, and bibliography, but it has no index.

BOGDAN C. NOVAK
University of Toledo

CARLO PAZZAGLI. *Per la storia dell'agricoltura toscana nei secoli XIX e XX: Dal catasto particellare lorenese al catasto agrario del 1929.* (Studi, number 25.) Turin: Fondazione Luigi Einaudi. 1979. Pp. 146. L. 3,500.

Carlo Pazzagli should be no stranger to students of Tuscan agriculture; he has already published a fine study on the subject, *L'agricoltura toscana nella prima metà dell'800* (1973). His most recent book, however, is a specialized monograph, the expanded version of a paper that he presented to a conference in 1977 on peasants and landowners in modern Tuscany. As the subtitle suggests, it involves a quantitative analysis of certain agricultural trends in the region during the nineteenth and early twentieth centuries through a comparison of the agrarian catasters of 1830 and 1929. The book opens with a critical discussion of the relative strengths and shortcomings of its primary source, the largely ignored *catasto agrario* of 1929. In order to ensure comparability between the two surveys, Pazzagli divides the region into homogeneous agricultural zones on the basis of changes in land usage and patterns of crop cultivation.

The two central chapters deal respectively with variations in agricultural production and livestock raising. The author's findings indicate that no fundamental transformation of the traditional agricultural system took place in the period between the two surveys despite a notable increase in the land under cultivation. On the contrary, they suggest a gradual and only partially successful adaption to market pressures through the diffusion and rationalization of the classic model of promiscuous *mezzadria*, or sharecropping, cultivation. The most striking progress took place in those zones that had been decidedly backward in the early nineteenth century, with a consequent narrowing of the distance between advanced and backward areas of Tuscany by the late 1920s. The virtual absence of industrial crops or specialized fruit cultivation as well as the extremely limited use of artificial fertilizers and motorized farm machines in 1929 further testify to the relative immobility of the region's rural economy well into this century. Agricultural intensification without corresponding technical innovation served to widen the gap between Tuscan agriculture and the modern commercial farming systems of northern Italy.

Pazzagli's findings largely confirm the familiar thesis that the extraordinary persistence of the *mezzadria* system in Tuscany precluded crop specialization and discouraged innovation, so that no complete transition to capitalist agriculture took place during the period. On the whole, the study represents less a finished work than an exploratory essay, and its primary value lies in its suggestions for future avenues of research. Specialists will also find

useful the statistical charts and maps that comprise nearly two-fifths of the book.

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UGO SPADONI. *Capitalismo industriale e movimento operaio a Livorno e all'isola d'Elba (1880-1913)*. (Biblioteca di Storia Toscana Moderna e Contemporanea, Studi e Documenti, number 21.) Florence: Leo S. Olschki Editore. 1979. Pp. ix, 458.

Like the authors of the other studies in this series, Ugo Spadoni has produced a solid monograph based on prodigious and encyclopedic research. The point of the book is to explain the reactions of the working class in Livorno, Elba, and Piombino to the painful and difficult process of industrialization that took place between 1880 and 1913. Within this narrow focus, Spadoni accomplishes his task. His contribution, however, would be even more useful if he had placed his results within the broader context of Italian economic and political developments and, especially, if he had compared the struggles of the workers in Livorno to those of the workers in other industrialized areas in Italy.

The author is particularly effective when he traces through the labyrinth of deals among industrialists, financiers, and politicians that marked the creation and growth of the steel industry in Tuscany. Spadoni's research into the complex, personalized economics and politics of the period substantiates the complaints lodged by authors as diverse as Giuseppe Are, Giorgio Candeloro, and Richard Webster that Italian businessmen were too cautious and unimaginative. His discussion of the leading industrial families in Livorno, and especially of the Orlando family, confirms that they, like industrialists in other areas of Italy, avoided risky ventures and used their political connections in the struggle to protect their interests.

As Spadoni explains, the continual division between reformists and revolutionaries weakened the labor movement and prevented it from presenting a concerted challenge to the industrialists and their political allies. It is also apparent, although Spadoni does not comment on it, that the working class in Livorno and Elba did not produce one important labor leader and that this lack of strong leadership may explain why the working class in Livorno and Elba appears so calm and disciplined in comparison to the workers in Turin, Genoa, or Milan.

Spadoni argues that the tactics of the anarchists and the revolutionary syndicalists confused the workers and, at the same time, increased the intransigence of the industrialists. The author shows a sympathetic understanding of the frustration of the

militants, but, intellectually, he supports the gradualist approach of Giuseppe Modigliani and the reformist socialists. He is especially good when he explains the dramatic and fatal strike in 1911 in the steel mills of Portoferraio and Piombino. Here, in microcosm, he presents all the contradictions that prevented the anarchists, the revolutionary syndicalists, and the socialists from uniting against the industrialists.

The author's excellent monograph is, unfortunately, marred by his extremely turgid style and his rather disorganized presentation. The book would have been better if he had not used lengthy quotations as a substitute for analysis and if he had included a brief, analytical conclusion. Even with its problems, however, the book makes an important contribution to our understanding of the development of industry and of the growth of the labor movement in Italy.

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R. J. B. BOSWORTH. *Italy, the Least of the Great Powers: Italian Foreign Policy Before the First World War*. New York: Cambridge University Press. 1979. Pp. x, 537. \$59.50.

R. J. B. Bosworth's severely critical analysis of Italian diplomatic aims and methods, mainly for the years 1910-14, contains many fresh insights and a considerable body of new information. This book merits serious attention even if some of the author's interpretations appear to be controversial. It originated in a Cambridge Ph.D. thesis on British policy toward Italy, 1902-15, and it contains a wealth of information from the Public Record Office, the Hardinge Papers, and the Rodd Papers. Research in the *Archivio Centrale dello Stato* has been especially rewarding, and Bosworth has used many high-level policy documents and political files in the *Archivio Storico* of the Italian Foreign Office. He does not, however, appear to have made any systematic examination of the telegrams through which much of Italian diplomacy was transacted. His bibliography of printed materials is the richest and most complete this reviewer has seen. The printed Russian documents, however, seem to have escaped his attention.

Strongly influenced by the work of Fritz Fischer on Germany, Bosworth concludes that liberal Italy's foreign policy differed mainly from that of Fascist Italy in method or style. Except for numbers and history, Italy, he finds, had more in common with a small Balkan state or a colony than a great power. The small elite who attempted to ease social tensions by having Italy play the expansionist role of a great power without much consideration of its mili-

tary and economic capabilities was essentially playing an unreal and dishonest game. Foreign Minister Antonino di San Giuliano, the Sicilian aristocrat who largely directed policy, has to be the chief villain in the story; the author finds him guilty of dishonesty in his statements and methods.

A short review is not the place for a debate on such complex issues. This reviewer's reading of the Italian documents leads him to feel that Bosworth is too severe in his indictment. Italian diplomacy before 1914 appears to have been as honest and able as that of any other great power. Italy's strategic position in that region where the Germanic, Latin, and Slavic people meet helped give it great power status. Who can condemn it for trying to live in peace and friendship with all of its stronger neighbors or for exploiting their rivalries in order to defend vital national interests in the Balkans and Mediterranean region by skillful diplomacy? Modern Italy's gravest errors have been its departures from this cautious policy of peace.

The book throws new light on Italian pressure groups, the foreign policy machine, Giovanni Giolitti's role in the Libyan War, the Austro-Italian rivalry in Albania, Italian friction with Britain and France, Italy's use of its occupation of the Dodecanese Islands to exert pressure for objectives in Albania and Turkey, and San Giuliano's cautious policy when faced with the war of 1914 but fails to describe fully Italy's major initiative to keep the peace. Chief of General Staff Alberto Pollio promised much more to Germany than two cavalry divisions in March 1914 (p. 217). The statement on German and Russian general mobilization should be checked for accuracy (p. 391).

WILLIAM C. ASKEW
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A. JAMES GREGOR. *Italian Fascism and Developmental Dictatorship*. Princeton: Princeton University Press. 1979. Pp. xv, 427. Cloth \$27.50, paper \$9.75.

A. JAMES GREGOR. *Young Mussolini and the Intellectual Origins of Fascism*. Berkeley and Los Angeles: University of California Press. 1979. Pp. xii, 271. \$16.50.

In these two books, the latest in a line of overlapping publications beginning with *The Ideology of Fascism* (1969), A. James Gregor repropounds the message that Mussolini was the real precursor of twentieth-century revolutions and his Fascist regime the prototype of anticapitalist modernizing systems everywhere in the world. In his own words, Italian Fascism anticipated "a class of regimes that includes Bolshevism, Maoism, Castroism, as well as an indeterminate number of other regimes that mobilize masses under both unitary party auspices and

the superintendence of charismatic and providential leaders, in the pursuit of modernization and development" (*Italian Fascism*, p. xii).

With the polemical fervor and intensity that characterize all his books on Fascism, Gregor develops a most idiosyncratic interpretation of Mussolini as a systematic and profound thinker who, before the end of the First World War, fashioned a persuasive doctrine of revolution based on the importance of revolutionary elites (an anticipation of Leninism), a synthesis of nationalism and socialism (an anticipation of Third World revolutions), and the correct conclusion that, in developing societies, the needs of production must take precedence over popular demands for wealth redistribution. All such insights into the nature of modern revolution Mussolini allegedly derived from his critique of classical Marxism, which, according to Gregor, he rejected and modified in the course of sustained intellectual labor spanning at least a decade—hence Gregor's description of Fascism as a heresy of classical Marxism and as the only viable revolutionary alternative for developing societies. Furthermore, in *Italian Fascism* Gregor argues that, once in power, Mussolini faithfully and coherently put into practice his doctrine of modernizing revolution, thereby transforming Italy into a major industrial power.

Such deliberately revisionist interpretations of Mussolini and Fascism will continue to be challenged, particularly by historians who do not share the political scientist's enthusiasm for political models and the arbitrary handling of documentary evidence that characterizes all of Gregor's writings on Fascism. Critics of his work have already identified certain obvious flaws, including the unwarranted notion that, when the Fascists came to power, Italy was an underdeveloped society that could serve as a model for Third World countries today. In fact, by 1922 Italy was already an industrial power, and, in many ways, the economic growth that occurred during the Fascist period was less impressive than that of liberal Italy. Anyone familiar with Fascist propaganda will immediately recognize a strange affinity between Gregor's interpretation and the image of their achievements that the Fascists wished to project. Gregor's Mussolini is not very different from the Mussolini who was always right, and his Fascism is essentially the Fascism of the trains that allegedly ran on time.

Although it is most unsettling to see the propaganda of yesterday reappear as the scholarly wisdom of today, any serious criticism of Gregor's work must transcend moral revulsion. His interpretation might have had some historiographical value twenty years ago when traditional views of Fascism as mindless social reaction, pathological behavior, or doctrinal nonsense still prevailed. Against such convenient straw men Gregor delivers mighty

blows. Since the mid-1960s, however, scholars have increasingly recognized that Fascism did incorporate a significant revolutionary strain, that it did raise serious questions about the nature of modern industrial society, and that some degree of popular consensus did form behind Fascist dictatorships. In this respect, the problem with Gregor's interpretation is simply one of balance, for he sees nothing but revolutionary Fascism and nothing but doctrinal revolutionary coherence on Mussolini's part. Such tunnel vision tells us nothing of how Fascism balanced revolutionary and conservative impulses, usually to the detriment of the former.

Carried away by his revisionist enthusiasm, Gregor takes unwarranted liberties with the evidence. A random check of his footnotes reveals some startling procedures. In *The Young Mussolini* Gregor asserts that Mussolini was already a nationalist in 1911 (p. 124), but the unquoted passage from Mussolini's *Opera Omnia* that supposedly supports this contention shows Mussolini writing in very plain language that "our denial of the bourgeois state would make no sense if we did not also have the courage to deny the fatherland [*patria*]." Although Gregor paraphrases this passage correctly, he chooses nevertheless to build his case on the most ambiguous reference to an "Italy purified," an expression that Mussolini does not use in that document. Misleading uses of documentary sources also appear on pages 78, 85, and 217 of the same book. Frequently the author uses the same material in different contexts, thereby creating the erroneous impression that his contentions are supported by a wealth of documentation. With such technique it should not be too difficult to fabricate a Mussolini for every occasion. Revisionists of the future take note, we still do not have a Mussolini the Feminist (but see *Italian Fascism*, pages 281–91), Mussolini the Military Genius, Mussolini the Consumer Advocate, Mussolini the Animal Lover, and Mussolini the Organic Gardener.

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PHILIP V. CANNISTRARO and GIANFAUSTO ROSOLI. *Emigrazione, chiesa e fascismo: Lo scioglimento dell'Opera Bonomelli (1922–1928)*. (La Cultura, number 21.) Rome: Edizioni Studium. 1979. Pp. xi, 260. L. 9,000.

This admirable monograph deals with a hitherto little-explored problem—the rivalry between Mussolini's Fascist regime and the Roman Catholic Church in the 1920s over looking after the interests of Italian emigrants in Western Europe. Gianfausto Rosoli is director of the *Centro Studi Emigrazione di Roma* and editor of the review *Studi Emigrazione/ Etudes Migrations*, while Philip V. Cannistraro of

Florida State University is the author of a number of studies on Fascist Italy, including *La fabbrica del consenso: Fascismo e mass media* (1974). They have conducted their research in the Italian Central State Archives, the historical archives of the Ministry of Foreign Affairs, and those of the *Centro Studi Emigrazione di Roma*. Renzo De Felice has contributed a preface. A long appendix contains texts of important documents.

An introductory chapter deals informatively with Fascist emigration policies—policies that were more complex than is commonly supposed. During the first five years of the regime more than 1,500,000 Italians emigrated. By 1927 there were 9,200,000 Italians living abroad, of whom 7,700,000 were in the Americas, more than 1,200,000 in Europe, and about 200,000 in Africa. Restrictions imposed by various countries in the 1920s as well as by the Fascist regime caused emigration to decline sharply. Giuseppe De Michelis of the *Commissariato dell'emigrazione* was the central figure in shaping Italy's emigration policies from 1919 until 1927. In 1923 Mussolini made De Michelis's commissariat a part of the Foreign Ministry and then in 1927 abolished it. That year marked a distinct shift in policy; Fascist political considerations henceforth took clear priority. Efforts were made to "fascistize" the previous emigrants. Although permanent emigration was to be halted, temporary emigration would still be tolerated. For political motives, the regime began to take over centralized financial and legal control of such private agencies as the *Opera Bonomelli*.

The *Opera Bonomelli* was founded in 1900 at the instigation of the bishop of Cremona, Geremia Bonomelli (1831–1914), who believed that the church should involve itself in temporal affairs. Its purpose was to furnish religious and moral assistance to Italian emigrants and to conduct patriotic activities among them. The association had a dual lay and ecclesiastical structure and a corps of missionary priests. The role of churchmen steadily increased at the expense of the laymen. Among the lay officials was Stefano Jacini, a Lombard who also became prominent in the new Catholic *Partito Popolare Italiano* that was founded in 1919 and was bitterly opposed by the Fascists. As early as 1923, Fascist pressure forced Jacini to resign his position in the *Bonomelli*.

The authors carefully show how Mussolini manipulated subsidies and personnel changes to bring the *Bonomelli* under Fascist control by 1927. Although annoyed by this, the Vatican did not wish to let this issue upset the more important negotiations then underway for the conciliation—negotiations that culminated in the Lateran Pacts of February 1929. Thus, in November 1927, Pius XI quietly dissolved the corps of *Bonomelli* missionaries, though not the *Opera* itself. Meanwhile, the regime

was secretly plotting to liquidate the latter in such a way as to avoid taking public responsibility for the action. The eventual solution was to have the *Opera* dissolve itself in July 1928. It is ironic that an organization founded by a churchman who sought reconciliation between church and state should be a victim of that very process.

The dictatorship's heavy-handedness in taking over the foreign facilities of the *Bonomelli* caused some deep splits within the emigrant communities. For this reason, one hopes that additional studies will be made of Italian working-class emigrants in France and elsewhere and their relationship to the anti-Fascist political emigration.

CHARLES F. DELZELL
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PAOLO POMBENI. *Il gruppo dossettiano e la fondazione della democrazia italiana (1938-1948)*. (Saggi, number 190.) Bologna: Il Mulino. 1979. Pp. 404. L. 10,000.

Paolo Pombeni has set for himself the task of tracing the origins and development of the so-called *corrente dossettiana* in the Italian Christian Democratic party. His book is divided into four chapters, the first two of which are concerned with the genesis of the movement in the pre-World War II period. The documentation for the book consists mostly of well-known printed sources.

Dossettismo takes its name from Giuseppe Dossetti, who began his career as a professor of canon law at the Catholic University of Milan in the 1930s. Unfortunately, the author does not succeed in presenting a very clear delineation of Dossetti the man, nor does he probe the roots of Dossetti's ideological convictions in any analytical fashion. It is difficult to understand why Pombeni refers to Dossetti as "charismatic." He may have been, but the evidence is not presented here.

The first chapter traces the earliest beginnings of *dossettismo*. A Milanese group of intellectuals, which included Dossetti, Carlo Colombo, Giuseppe Lazati, and Amintore Fanfani, sought to update Catholic social doctrine in order to respond to the social crisis of the age. Pope Pius XII's Christmas message of 1942 and the fall of Mussolini in 1943 further stimulated their thinking on the role of Catholicism in the modern world.

The second chapter traces the Dossetti group from 1943 to 1946. This chapter devotes an excessive amount of space to the activities of virtually all Catholic political groups of the Resistance period. According to the author, Dossetti opposed a separate Catholic party and urged Italian Catholics to join the political parties closest to their own ideas. Pombeni fails to explain satisfactorily why Dossetti chose to enter the Christian Democratic party or

why he was chosen as the first national vice secretary of the party. Pombeni is at his best in describing the ideological and methodological differences between Dossetti and De Gasperi, the head of the Christian Democratic party. It was during the debates over the "institutional question" that the *corrente dossettiana* took shape.

The third chapter is an exhaustive analysis of the *dossettiani* during the drafting of the constitution for republican Italy. The most interesting sections of this chapter deal with Dossetti's attitudes toward the Lateran Pacts, which became a part of the constitution. There is also a detailed description of the views of the *dossettiani* on socioeconomic matters.

The final chapter has the rather strange title of the "Birth of *dossettismo*"—strange because the chapter covers only the year 1947 although Pombeni presents Dossetti as a strong leader with definite convictions prior to 1947. In discussing the *dossettiani*, Pombeni contends that they did not represent a homogeneous group, although they took their economic views from Keynes. The birth of *dossettismo* went through various phases, culminating in the publication of *Cronache Sociali*. By this time the *dossettiani* were considered part of the left wing of the Christian Democratic party. The political events of 1947, in the judgment of Pombeni, clearly indicated that the group would have no discernible impact on the hierarchy and program of the Christian Democratic party. Pombeni does not carry the story of *dossettismo* beyond the end of the experiment in tripartism. The book would have been more valuable if the story of the movement had been covered at least until 1951, when Dossetti retired from politics. The book will be read with profit by scholars interested in a general history of the most important "current" in the early Christian Democratic party.

ELISA CARRILLO
Marymount College

JAROSLAV ŠIDAK. *Studije iz hrvatske povijesti za revolucije 1848-49* [Studies in Croatian History during the Revolution of 1848-49]. Summary in German. Zagreb: Sveučilište u Zagrebu—Institut za Hrvatsku Povijest. 1979. Pp. 394.

In 1948 Croatian historians celebrated the centennial of the revolutions of 1848-49, which was marked by the appearance of several published works, including two books by the historian Vaso Bogdanov. There were still then historians who had not freed themselves of Marxist rigidity, Bogdanov being one of them. Croatia in the revolutions of 1848-49 has long fascinated the historian Jaroslav Šidak, and, especially after the centennial publications, he decided to investigate the subject in an exacting manner. When Šidak questioned the va-

lidity of Bogdanov's theses at the first meeting of Croatian historians, held on November 26, 1955, in Zagreb, he had the backing of most Yugoslav historians. He particularly challenged Bogdanov's labeling as "communists" the followers of the so-called "left" (*ljevice*) in order to make them appear more "progressive" than they actually were. At most, he said, the individuals of the "left" could be referred to as "radical democrats" or progressives. Šidak noted that a careful reading of the "complete" texts of articles written by Dragutin Kušlan and two other "radical democrats," Ivan Kukuljević and Bogoslav Šulek, showed that they condemned the October Revolution in Vienna, took the side of *ban* Josip Jelačić (Jellačić), and backed Austroslavism in much the same way as did the Czech radical democrats and the liberal bourgeoisie, considering it as the only possible formula for the solution of the nationality question within the Habsburg Monarchy.

Over the years Šidak wrote a number of essays on the revolutions of 1848–49 in which he challenged not only Bogdanov's views on Croatia in 1848–49 but also in a less dramatic way some conclusions and observations advanced by certain other authors (A. Jelačić, R. Bičanić, St. Gavrilović, and I. I. Leshchilovskaia). Seven of these essays, four not previously published, have been put together and issued in book form. The author selected only those essays that deal with Croatia proper. In some instances the republished materials are improved stylistically with footnotes expanded to include reference to additional sources. Two essays have been shortened.

A nine-page statement in which Šidak explains the purpose of his book and the methodology employed is followed by essays on the significance of the revolutions of 1848–49, the March 25 demands, relations between *ban* Jelačić and the principality of Serbia, Austroslavism, the society *Slavenska lipa* (modeled on the Czech *Slovanská lipa*), and an essay on the historiography of the Croatian movement of 1848–49. In discussing these broader topics, the author makes some keen observations about Jelačić's military operations against the Magyars, the attitude of the Czech and Croat radical democrats toward the October Revolution, the journal *Slavenski Jug* (edited first by Dragutin Kušlan and later by Bogoslav Šulek, until its prohibition in February 1850), the first and the second Croatian deputations to the king, the convocation of the Croatian diet, and the appointment of Jelačić as supreme commandant in Croatia.

By selecting the best from among many fine articles (including some fascinating ones on individual Croat leaders) that Šidak published in a number of journals and in the *Encyclopedia of Yugoslavia*, he has given us a significant contribution to the historiography of the revolutions of 1848–49. Of particular

value is his assessment of the sources for and monographic studies on Croatia in 1848–49 and his pointing out the direction for further research. Šidak has paved the way for some historian to produce a badly needed synthesis of Croatian history of the turbulent years of 1848–49, although no one is more qualified to write it than Šidak himself. A lengthy bibliography, an index of persons, and a list of illustrations add to the value and quality of this important work.

WAYNE S. VUCINICH
Stanford University

DUNCAN WILSON. *Tito's Yugoslavia*. New York: Cambridge University Press. 1979. Pp. xii, 269. \$27.50.

Now that the time of testing for post-Tito Yugoslavia is at hand, it is more important than ever for those who have an interest in what happens—and that includes the governments and peoples of the West—to have an understanding of the Yugoslavia that Tito's leadership did so much to create and of the legacy that he has left. Duncan Wilson had distinct advantages in setting out to contribute to that understanding: first, through service as British ambassador to Yugoslavia he knows the country's leaders, institutions, and culture at first hand; second, his diplomatic career has also included service as chief of mission in Peking and in Moscow, giving him a broad perspective on the international politics of the Communist world; third, as a scholar he has read widely and critically in background material, Yugoslav and Western.

Thus, *Tito's Yugoslavia*, covering 40 years of turbulent history, is, as you might expect, sound, balanced, urbane, diplomatic, sympathetic, but not naive. It cannot be comprehensive and skips rather lightly over some problems and episodes, but Wilson proves himself a master of the demanding art of writing succinctly, without distortion and without going into all the qualifications and complexities about which "the reader with a general interest in the contemporary world" for whom he writes does not wish to be bothered. Of course, those complexities are what fascinate the specialists, and they will not find much new here by way of fact or interpretation, no revelations of secret diplomacy or of intelligence that must have been available to an ambassador; Wilson remains the discreet diplomat. But he can be confident that his book will stand up to the specialists' critical review.

Of his interpretations, two deserve special mention. One is the continuing interaction of domestic and foreign policy. It exists in almost any country, but with Yugoslavia it has become an organic connection because the issues of socialism, both as to internal order and for foreign policy, involve mat-

ters of heresy, excommunication, and life and death; the love-hate relationship with the Soviet Union is always at the center of Yugoslav concerns. Further, in Yugoslav eyes the road of socialist self-management and the road of nonalignment are enveloped in a common mythology, which outsiders would do well to try to understand.

The other interpretation gives a consistency to Yugoslav internal developments not often apparent on the surface. Thus, the course of events since 1945 shows shifts and reversals, first in the direction of central controls and coercive methods, then in the direction of relaxation, experimentation, and reform. Wilson sees these not as a succession of sharp changes, but as an accumulating variety of experiences, none of which is entirely discarded and all of which survive in one form or another, creating a pluralism of sorts that makes the Yugoslav reality hard to describe clearly and sharply at any given time but also makes it difficult for the leadership to move rapidly forward or backward through new decisions. The society, in other words, has created weights and balances of its own, even though they are not the ones written into the constitution.

That pluralism gives today's Yugoslavia certain strengths that may serve the country well in the future against pressures that are bound to come from outside and from inside. Tito was the indispensable man at critical points of Yugoslavia's experience, but that does not mean the country will succumb without him. This book shows very well what reserves of strength there are for independence and for unity. Tito built on those strengths, though he did not create them, and they remain.

JOHN C. CAMPBELL
Cohasset, Massachusetts

TAHSIN GEMIL. *Țările Române în contextul politic internațional (1621–1672)* [The Rumanian Principalities in the International Political Context (1621–72)]. Summary in English. (Institutul de Istorie și Arheologie "A. D. Xenopol" Iași "Biblioteca Istorică," number 52.) Bucharest: Editura Academiei Republicii Socialiste România. 1979. Pp. 231. 11 L.

This valuable diplomatic history of Eastern Europe—focusing on the Rumanian principalities of Moldavia, Wallachia, and Transylvania in the mid-seventeenth century—is rather complicated but also highly illuminating. There is an unevenness in Tahsin Gemil's story due to the difficulties inherent in blazing a scholarly trail as well as to the disparate political situations in the three Rumanian principalities. Gemil stresses Rumanian unity in confronting the national self-interest (as in recurring attempts at territorial aggrandizement) of the leaders of the Polish republic, Turkish empire, and Cri-

mean Tatar khanate. Rumanians in each principality wanted national unification and political independence, and they therefore cooperated with other Christian states in attempting to overthrow Turkish political domination. Yet, paradoxically, the survival of the Rumanian principalities depended on Rumanian leaders' continued recognition of the Ottoman sultan's suzerainty as well as on the cultivation of Polish and Austrian assistance in maintaining the regional balance of power.

Especially helpful is Gemil's discussion of the policies and motives of the Turks, Poles, Crimean Tatars, and Moldavian and Transylvanian Rumanians; he gives somewhat less attention to the Wallachian Rumanians and considerably less to the Austrians, Russians, and Ukrainian Cossacks. Where documentary evidence is absent, Gemil warns about the conjectural nature of his observations, and, indeed, not all of his thoughts are convincing. For example, he provides scant support for contentions that Russia launched "anti-Orthodox propaganda" for the liberation of southeastern Europe (p. 152), that Prince Gábor Bethlen of Transylvania sought to recreate a Dacian kingdom (p. 68), and that Prince Vasile Lupu of Moldavia hoped to lead a resurrected Byzantine empire (p. 122). Gemil does, however, offer a fresh interpretation of Vasile Lupu as a farsighted leader who endeavored, albeit unsuccessfully, to unite the Rumanians (p. 114). Also significant is the author's evaluation of the Crimean Tatars as primary agents of Turkish diplomatic and military affairs in Eastern Europe.

Historical literature on this theme, according to Gemil, is practically nonexistent in Rumanian, Polish, and Turkish—and the author only sparingly consults the more abundant Russian writings. Much of his study rests on manuscripts in the archives and libraries of Istanbul, Warsaw, Cracow, and Bucharest. May we hope he will continue his research and provide us with another volume describing the Rumanians' role in subsequent European diplomatic relations, especially during the late seventeenth-century wars for hegemony in Eastern Europe. Such a monograph would be a fitting capstone to the fascinating work at hand.

FREDERICK KELLOGG
University of Arizona

BIANCA VALOTA. *Questione agraria e vita politica in Romania (1907–1922): Tra democrazia contadina e liberalismo autoritario*. Milan: Cisalpino-Goliardica. Pp. xxii, 273. L. 8,000.

Between 1907 and 1922 far-reaching agrarian reforms were enacted in Rumania; at the end of 1918 universal male suffrage was introduced. These measures were intended to stave off revolution. In radi-

cally changing the country's economic and political structures, however, they served further to disrupt the all-important agricultural sector (badly affected by the Great War) and threatened the stability of government itself. Rumania's problem was a perennial one of the modern age: how a backward country can modernize its economy and society rapidly and safely while retaining a viable democratic political system.

Rumania started with better chances than many. It had some experience of national independence, possessed a westernized elite, and had emerged a "victor" from the war (with its territory and population doubled). Nonetheless the attempt failed. In this book Bianca Valota describes the process and analyzes the failure. In doing so she draws on a wide range of sources including archival material (mostly on peasant disturbances). A strict conceptual framework of a popular Marxist kind helps her to produce a compressed and intelligible account out of the mass of information she has collected on agrarian legislation and political events, though it sometimes induces her to lapse into stereotype (rapacious landlords, profiteering bourgeoisie, wicked foreign capitalists). That said, and even though her sympathies are obviously with the poor peasants, her shrewd historical common sense shines through. She may depict the Machievellian Ion Brătianu as the villain of the piece, but she admits that no other politician had sufficient nous to handle the postwar situation.

Overall, the book presents a realistic appreciation of Rumania's problems: an economy over-dependent on the international grain market, the difficulty of integrating the new territories, the innocence of the new political leaders, the corrupt bureaucracy—but above all, perhaps, the sheer weight of Rumania's past. Demography is neglected (the population increase of over 20 percent between 1899 and 1914 and the age structure of postwar Rumania were not unimportant factors); the Jews (who occupied a singular and unenviable place in the Rumanian scene) and the church are hardly mentioned. But otherwise the book provides a rounded picture of Rumania's predicament, its leaders' perceptions of it, the proffered solutions, and their material effects.

In the end "peasant democracy" proved to be a chimera, not least because the peasants themselves were both economically conservative (they demanded land but rejected cooperatives) and politically mercurial. The upshot was a resort to authoritarianism. The story conveys a sense of tragic inevitability, but the telling of it provides some valuable insights into a crucial period of Rumanian history and should enhance understanding of subsequent developments in Eastern Europe as a whole.

PHILIP LONGWORTH
McGill University

KARL REINERTH. *Die Gründung der Evangelischen Kirchen in Siebenbürgen*. (Studia Transylvanica, number 5.) Cologne: Böhlau Verlag. 1979. Pp. xi, 348. DM 88.

In Transylvania and the counties of eastern Hungary administered by the Transylvanian princes during the sixteenth century, Lutheran, Helvetic, and Anti-Trinitarian theologies gained adherents among the German and Magyar population. In November 1571 the Diet of Ncumarkt (Marosvásárhely, Tîrgu Mureş) recognized all of these reform movements together with Roman Catholicism as "established" confessions, legally tolerated in the principality. Eight months later, in June 1572, the evangelical pastoral conference of the Siebenbürgen Saxons, under the leadership of Lukas Unglerus, formally accepted the Augsburg Confession of 1530 and their own *Formula pii consensus* as the doctrinal standard and ecclesiastical symbol of their churches. These two events can be considered as the *terminus ad quem* of the first half of the Reformation era in Transylvania, which began early in 1520 in Hermannstadt (Nagyszeben, Sibiu) but which gained greater impetus with the labors of the layman and humanist, John Honter of Kronstadt (Brassó, Braşov).

During the twentieth century, several major studies have attempted to decipher and depict the complex course and interrelationships of the various reformist movements in Transylvania, of which those by Friedrich Teutsch, Adolf Schullerus, Karl Kurt Klein, and Erich Roth are most accessible to Western scholars. These works have emphasized, individually or collectively, the Teutonic-linguistic, humanist, and Helvetic influences upon the reformers in Transylvania.

In this study, Karl Reinert, the foremost modern scholar of the Reformation in Siebenbürgen, offers a synthesis of his own numerous investigations published during the past half century and his thorough acquaintance with the sources and scholarly literature. He attempts to revise current perceptions of the Transylvanian Reformation and to validate an earlier tradition of interpretation that maintained the preeminence of the theology and ecclesiology of the Wittenberg reformers, especially Melancthon, for the Saxon and, until 1555, even the Magyar reformers in Transylvania. Reinert does not deny that, insofar as the Reformation in Transylvania is perceived as grounded in the work of John Honter, its roots can be traced to Augustinianism, humanism, and the thought of the Basel reformers. As a humanist and Augustinian, Honter sought to reform the worship and life of the church in Kronstadt and to cleanse it of the abuses that he believed had resulted from the extensive ecclesiastical and political power of the clergy. His adoption of the principle of *sola scriptura* was as much the natural response to the humanist rallying cry, *ad fontes*,

that he came to know at Vienna and Cracow as it was the result of the influence of the Basel reformers, especially Oecolampadius and Bonifatius Amerbach, whose moderate formulations molded Honter's theological attitudes and principles. Nevertheless, after 1541 Honter exhibited an increasing reliance on the formulations of the Wittenberg reformers while, after 1555, the center of reform among the Saxons shifted to the Lutheran clergy of Hermannstadt, especially Paul Wiener, the first superintendent of the Transylvanian Lutherans, and his successor, Lukas Unglerus. At the same time, the Magyars increasingly identified with the Helvetic theology advocated by the clergy of Debrecen or even with the Anti-Trinitarianism propounded by Italian émigrés and Francis David. The increasingly Lutheran character of the Reformation among the Saxons of Siebenbürgen, manifested by the influence of the *Confessio doctrinae Saxonicarum ecclesiarum* of 1551 and the emergence of Hermannstadt as the Transylvanian Wittenberg, was accompanied by the reemergence of substantial clerical authority in the Lutheran Church.

Although Reinert's treatment of the Reformed and Anti-Trinitarian movements among the Magyars is less extensive and effective than his study of the Lutheranism of the Saxons, this careful and persuasive study, a greatly expanded version of his earlier *Die Reformation der siebenbürgisch-sächsischen Kirchen* (1956), is the best recent analysis of the theological development of the evangelical Lutheran Church in Transylvania.

DAVID P. DANIEL
Concordia Seminary

LADISLAV LIPSCHER. *Die Juden im slowakischen Staat, 1939-1945*. (Veröffentlichungen des Collegium Carolinum, number 35.) Munich: R. Oldenbourg Verlag. 1980. Pp. 210.

The history of small nations is not marketable; students of such annals need not apply for jobs. It is, therefore, a comforting surprise to see the publication of a volume devoted to a numerically insignificant minority within a small nation.

Perhaps because of the size of Slovakian Jewry—equal to a moderately large ghetto in Poland—students of the Holocaust have displayed only a marginal interest in its fate. Yet, Ladislav Lipscher, a native of Czechoslovakia and a resident of Switzerland since 1970, shows that a microcosm can offer a valid sample of occurrences taking place on a larger scale and that inner strength cannot be measured by numbers only. This ably written study demonstrates these arguments.

The author concentrates on the Slovak state's policies toward its Jewry. The various influences that shaped these policies and the implementation

of the decided-upon measures are at the focus of the book. The influence of the Third Reich was decisive, the author claims, although local radicals instigated the persecutions. Slovak moderates were, with several honorable exceptions, timid and shy of involvement. Lipscher provides an elaborate picture, replete with unknown details. His conclusions are debatable, however, and putting the blame on the Nazis seems to wipe other hands clean. The discussion is based both on massive evidence obtained in Czechoslovak archives and on interviews.

Developments inside the persecuted community are dealt with only in a rudimentary fashion. We learn little about Slovak Jewish communal and individual life. Although the writer devotes space to activities of the Jewish underground leadership and the Jewish resistance, he neglects to place Jewish efforts to help themselves and others (like the Greek, Polish, and Hungarian Jewries) into a proper perspective. Here his sources are deficient and unsatisfactory.

In his introduction, Lipscher asserts wrongly that there were few relevant documents and publications. The appearance of numerous works in the seventies calls for at least a partial revision. Furthermore, the author refers to only a single volume of the Vatican's documents. Important archives were left unconsulted. In particular, the Central Zionist Archives in Jerusalem is a *sine qua non* for the above topic. Recorded testimonies of surviving Jewish leaders were also overlooked. Some published and unpublished memoirs and diaries were not used. Without these extensive sources one can hardly describe the efforts at salvation of and by the Slovak Jewry or the internal life and behavior of congregations and individuals.

In 1961 Livia Rothkirchen published a pioneering although already dated book on the destruction of Slovakia's Jews. She compiled her volume from information available in Western archives. Subsequent publications have used similar material. Lipscher, on the other hand, had the rare access to Czechoslovak and East German archives. Only an integration of these two sources will make possible the writing of a definitive monograph on the ordeal of Slovakian Jewry. Lipscher has advanced our knowledge considerably. Nevertheless, his study constitutes but one stage in an unfinished undertaking.

YESHAYAHU JELINEK
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A. M. OREKHOV. *Stanovlenie pol'skogo sotsialisticheskogo dvizheniia: Struktura, programnye kontseptsii, deiateli, 1874-1893* [The Formation of the Polish Socialist Movement: Structure, Program Concepts, and Leaders, 1874-93]. Moscow: Nauka. 1979. Pp. 373. 2 r. 60 k.

A. M. Orekhov, a Soviet historian specializing in nineteenth-century Polish revolutionary movements, here presents an analysis of the formative period of socialism in Poland—the years 1874–93. The point of departure coincides with the date of the Russian Narodniks’ “going to the people,” an event that stimulated Polish students and intelligentsia to shake off the passivity of the post-1863 period. Although inspired by the example of the Russian revolutionaries, the Poles were from the start more inclined toward the scientific rather than the utopian brand of socialism. The initial phase consisted of agitation on the level of debating circles and workers’ educational or self-help associations. By 1878 groups formulating political programs began to emerge, and in 1882 Ludwik Warynski founded the first socialist party, the Proletariat. The ruthless suppression of the Proletariat in 1884 resulted in a temporary disruption of socialist activity, which was soon resumed and carried on by other groups. The years 1887–93 saw a crystallization of two major currents within Polish socialism: Marxist-internationalist and reformist-nationalist. The period under discussion ends with the split in the ranks of the newly created Polish Socialist Party (PPS) from which the internationalist wing led by Rosa Luxemburg later emerged to form the Social Democratic Party of the Kingdom of Poland (SDKP).

The Soviet historian’s sympathies are clearly on the side of the latter party, which put the cause of revolution throughout the Russian empire above Polish national aspirations. He is conscious, however, of the particular significance of these aspirations in a country deprived of its independence. He points out that the somewhat ostentatious rejection of the struggle for the restoration of Poland by the early socialists was a shortcoming rooted in the simplistic belief that the revolution would solve the national problem automatically. The same criticism can be applied to the most important party of the period, the Proletariat, which agreed to subordinate itself to the Russian People’s Will organization. The book does not gloss over the difficulties inherent in the relationship between the romantic-terrorist Russian group, whose peasant-oriented ideology reflected the condition in pre-industrial Russia, and the largely Marxist, working-class-based Polish party, which was a product of a rapidly changing Polish society. Antipatriotic though it was, the Proletariat took precaution to stipulate that its subordination would not last beyond the overthrow of tsardom. Russian Narodnik influence on the nascent socialism in Poland must be judged negatively insofar as it encouraged terrorism, anarchism, and a conspiratorial approach to political action.

Orekhov’s treatment of his topic is on the whole objective and free of preconceptions, although the tenor of his narrative changes markedly in the last

chapter of the book, which deals with a more controversial issue, the rise of the PPS. He presents the broad background of circumstances that led to this event, generally recognized as the turning point in the history of Polish socialism. Still, he resorts to the stereotypical interpretation of the 1892 Paris Congress as a triumph of separatism, nationalism, and reformism, all attributed to petty bourgeois influences. This nebulous term explains little, but it does suggest that the author is treading on uncertain ground. The reader might well wonder why the socialists of the 1880s are criticized for their disregard of the national question, while the PPS, with its call for an independent Polish republic, deserves harsh condemnation. It certainly does not follow that Rosa Luxemburg’s SDKP, for all its Marxist correctness, took a proper stand on this particular issue.

The closing chapter excepted, Orekhov’s book remains a solid, analytical review based on archival sources and abundant literature. The lack of bibliography is all the more striking in this otherwise generally high-quality work.

TADEUSZ SWIETOCZOWSKI
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ANDRÉ LIEBICH. *Between Ideology and Utopia: The Politics and Philosophy of August Cieszkowski*. (Sovietica, number 39.) Boston: D. Reidel. 1979. Pp. vii, 390.

ANDRÉ LIEBICH, editor and translator. *Selected Writings of August Cieszkowski*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1979. Pp. vi, 174. \$23.50.

In the 1830s August Cieszkowski (1814–94), a disciple of Hegel, challenged Hegel’s four-period division of history and proposed in its place a dialectical triad that included, as its third component, the future. Cieszkowski argued that “the organic and ideal entirety or the apodictic process of history” could not be known “without *knowability of the future*, that is, without considering the future as an integral part of history.” While Hegel had been content to contemplate the world, Cieszkowski insisted that the task of the new period of history, the era of *deed*, was to achieve the synthesis of “thought and being.” Thus, Cieszkowski took the position that Marx later adopted in his “Theses on Feuerbach” when he proclaimed that “the philosophers have only *interpreted* the world . . . the point, however, is to *change* it.”

Cieszkowski’s place in the prehistory of Marxism is recognized (Cornu, Lobkowicz, Avineri, Lichtheim, and others) as is his influence on Russian thought (Martin Malia’s biography of Herzen). André Liebich does much more. *Between Ideology and*

Utopia is a full intellectual biography of this Polish nobleman, who owned estates both in Congress Poland (then under Russia) and in Prussian-ruled Poznań and was educated in Cracow and Berlin. Cieszkowski wrote on philosophy in Latin and German, on economics and politics in French, and on religion, current affairs, and other subjects in Polish. He was also a promoter of modern agriculture, an organizer of scholarship and the press, a busy politician, and a member of the Prussian diet.

Liebich does not think that Karl Mannheim's famous distinction between "ideology" and "utopia" applies to the thought of Cieszkowski, which, he argues, contained elements that could be classified as ideological along with those that were "utopian." Cieszkowski was neither a backward-looking conservative nor a forward-looking socialist, although he was a critic of many features of capitalism. The conventional dichotomy of left and right simply does not fit him.

Between Ideology and Utopia consists of three parts: Germany and Philosophy, France and the Social Movement, and Poland and Messianism. Each part presents first the basic biographical data, followed by a detailed analysis of the pertinent writings of Cieszkowski, and concludes with a review of the reception of his ideas and their impact. The same structure lies behind the editor's introduction to the *Selected Writings of August Cieszkowski*. The latter volume is a brief but representative selection of Cieszkowski's works from the "German," "French," and "Polish" phases of his life. One may suppose that it will attract a wider audience than the more technical and longer monograph and that it will help the reader to understand, to quote Liebich, "how Cieszkowski can be seen as both a social thinker of prosaic and partial innovation and an exalted prophetic figure of grand proportions." While Cieszkowski's "attempt to enlist Providence as history in the service of reform," which is how Liebich summarizes his work, was unsuccessful, the Polish philosopher advanced many ideas that have survived him. Cieszkowski tried to reconcile Christianity and progress, and the Polish intellectual historian, Andrzej Walicki (whose work Liebich knows), was right in arguing that Cieszkowski's *Our Father*, for example, anticipated the ideas of the Catholic *aggiornamento* of our time. On a different plane, Cieszkowski emerges as an influential political and social activist who helped to mold the political culture of Prussian Poland, which became known for its strong commitment to economic progress and social reform in combination with an attachment to Catholicism and a concomitant immunity to socialism and populism.

ROMAN SZPORLUK
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PETER RAINA. *Stefan Kardynał Wyszyński Prymas Polski* [Cardinal Stefan Wyszyński, Polish Primate]. Volume 1. London: Poets' and Painters' Press. 1979. Pp. 578.

This well-timed book provides an occasion to have another look at an issue of paramount importance in shaping postwar Polish life—namely, the problem of church-state relations. Peter Raina's study furnishes not only an opportunity but also rich material for consideration of this subject, for the volume is not only—as the title may suggest—a biography of Cardinal Stefan Wyszyński but also a history of the Catholic church in contemporary Poland as well.

When a writer tries to combine in a single publication a biography, a historical account, and a documentary collection, it is easy for critics to carp, to point out various omissions, and to deplore the uneven tempo of his narration. And yet, just as the pace gets too slow and the subject matter too thin, it suddenly comes through all the welter that the author is consistent in exploring the basic problem.

Raina asserts that Primate Wyszyński considered the establishment of a *modus vivendi* between the church and state as his major concern. Unfortunately, the assumption of his primateship (February 1949) coincided with the ascendancy of the Stalinist period of indoctrination, intimidation, and coercion. Throughout this period, as well as in later years, the attitude of the primate, and of the episcopate as a whole, toward the Communist rulers was conciliatory, although firm and confident. This behavior is explained by the fact that the church in Poland saw itself as the defender of the faith and of the nation. When there was danger that the escalation of domestic conflict might lead to Soviet intervention, the primate was willing to use his influence and act in consonance with the Communist Party.

While showing remarkable restraint and the willingness to compromise, the primate not only defended the church's institutional interests but also stubbornly fought for the preservation of the national heritage and in defense of human rights. In this way, due to circumstances, the church became a sort of parapolitical opposition, the only ideological force to challenge the totalitarian regime. No wonder that the church, and the primate personally, became once again the focus of popular hopes and aspirations. At the same time representatives of the "Secular Left," ascertains Raina, "duplicated party slogans and wrote poems in praise of Stalin" (p. 464).

It is truly remarkable that the author, whose background, after all, inclines him to adopt a posture of disengagement, shows so much concern and sympathy for the hero of his book. I suppose that the decisive element in his attitude is not so much

the magnetic personality of the primate (to which I can attest) but rather the writer's sound orientation in Polish affairs. Raina, a native of India, spent five years at Warsaw University, where he acquired both a Ph.D. in history and an impressive amount of information on contemporary Polish affairs. Returning to the West, he continued his interest in Poland and published extensively on Polish subjects. The book under review is the first volume of an extensive study on Cardinal Wyszyński. It covers the period of his early life and ends with his arrest in 1953.

The book is published with great care and does credit to the printing art. A selective bibliography and extensive index add to the value of the volume.

V. C. CHRYPINSKI
University of Windsor

VS. I. OSTOL'SKII and A. A. CHEKANOV, editors. *Ocherki istorii tekhniki v Rossii s drevneishikh vremen do 60-kh godov XIX veka* [Essays on the History of Technology in Russia from Earliest Times until the 1860s]. Moscow: Nauka. 1978. Pp. 374. 2 r. 60 k.

This is the first in a group of four volumes that will study the history of technology in Russia from the earliest times to the Bolshevik Revolution. I. I. Artobolevskii is chairman of the editorial board for the entire series. Vs. I. Ostol'skii and A. A. Chekanov are the editors-in-chief of this collection, a gathering of six essays that begins with Paleolithic and Neolithic times and ends with the 1860s, the point at which most Soviet historians hold that the Russian empire finally made the transition from feudalism to capitalism. The foreword credits nineteen scholars for the exact portion of a chapter that each has contributed. The principle of collective authorship combined with the many editorial stages through which these essays have passed produces a standardized treatment of each historical question and a corresponding loss of the sense that any particular passage emanates from a specific individual who has wrestled with the subject matter and emerged from it with gaps and questions as well as secure knowledge. What is gained from this approach, however, is a steady and authoritative rendering of Soviet knowledge about a vast, complex field that has long occupied the attention of some of the nation's finest scholars.

The first chapter of part 1 is entitled "Technology in the Earliest Period." Based upon archeology and Engels's theories about primitive communism, this is the briefest (31 pages) and most hypothetical chapter in the book. A continuing use of archeology with an imaginative use of written sources places the second chapter upon a sounder scholarly foundation. "Technology in Ancient and

Medieval Russia" discusses farming implements, salt and iron production, mechanical clocks, saltpeter and gunpowder production, utilization of chemicals, the building of roads and bridges, military implements—to mention but a few subjects.

The second part (entitled "Technology in Russia from the Seventeenth Century to the 1860s") is divided into four chapters dealing in succession with mining, metallurgy, energy, and machine building. These pages form well over two-thirds of the book's contents and contain valuable discussions of such subjects as iron artisan and manufactory technology, coal mining and oil extraction, metallurgy (including the refinement of precious metals), dam technology and water power, hydraulic motors, steam power plants and engines, the development of a theory of heat by Russian scientists, machine building, iron industry technology and foundry processes during the early nineteenth century, metal cutting tools, lifting and carrying devices, machinery construction and design, and, finally, the development of a theory of the machine.

Although most of the subjects discussed in the last five chapters receive adequate treatment, the effort to compress so much material into so few pages necessarily produces some very sketchy accounts. "The Theory of Machines in School Pedagogy and Popular Publications" during the last two decades of the eighteenth century, for example, is disposed of in twenty-seven lines, while "Theoretical and Applied Mechanics" in the same period does not even fare this well. But the foreword makes clear that this book is intended for a "popular audience" (which in Soviet terms may indicate a rather serious group of readers), and the data sheet at the end indicates that it has been issued in 7,550 copies. These *ocherki* are lavishly illustrated with ninety-seven sketches and sets of diagrams, plus many interesting portraits. The volume contains a five-page bibliography that begins with the works of Marx, Engels, and Lenin on the subject (viewed, admittedly, in a broad perspective) and goes on to list 139 secondary studies and published source collections from the pre-1917 and Soviet periods. There is a name index and a subject index.

Essays on the History of Technology in Russia is a craftsmanlike work largely free of the exaggerated nationalism that marred so many earlier Soviet studies of this type. Yet it does lead one to see that Russians at nearly every stage of their history have been a reasonably "technological" people and that many interesting innovations and innovators have emerged from their ranks. If the volume suffers occasional provincialism and never really wrestles with the question of how "advanced" Russia was at various times in comparison with other societies, its merits are great enough for one to hope that it might someday appear in the Soviet Union in Eng-

lish translation. Many people outside the USSR would read it with profit, indeed, surprise.

JOSEPH T. FUHRMANN
Murray State University

L. I. IVINA. *Krupnaia votchina severo-vostochnoi Rusi kontsa XIV-pervoi poloviny XVI v.* [The Large Landed Estate of Northeastern Rus at the End of the Fourteenth through the First Half of the Sixteenth Century]. Leningrad: Nauka. 1979. Pp. 221. 1 r. 80 k.

The Simonov Monastery, which managed extensive landholdings around Moscow, has maintained a nearly complete documentation record. Liudmila Ivanovna Ivina presents in a methodical and pedestrian way a verbal picture of that monastery's land growth from the fourteenth to the sixteenth century. She makes extensive use of published and unpublished charters, as well as the copy, donation, and tax books of the monastery. Through an enormous effort she has extrapolated from the extant documentation detailed information concerning settlements and villages, immunities, and privileges.

Another way of treating the various documents would have been to apply the principles of diplomacy in order to ascertain differences and nuances in wording and formulas. Such information would also be valuable. One might quibble that the fold-out map of the landholdings has a befuddling system of numbers and symbols. And one would like to have seen a few charts and graphs presenting the extensive information more compendiously.

But my main argument with Ivina's book is her conceptual frame of reference, which appears not to derive from her sources but to result in squeezing the evidence into an already constructed interpretive box. A case in point is the question of secularization of church and monastic lands by the grand prince, whose influence Ivina sees occurring early: "Thus, from the 1480s to the beginning of the sixteenth century—in the period of the government offensive against monastic lands—the Simonov Monastery's patrimonial estate rose slowly" (p. 126). She almost invariably attributes any "slowing of the tempo of the rise of monastic landowning" to government interference (pp. 103, 119, 153, 169). This, despite the fact that no evidence of direct government interference can be cited. Many possible reasons for the slowing of the pace of land acquisition could be considered instead, such as demographic, economic, religious-spiritual, and so forth.

She also explains the increase in the number of peasant complaints and examination of them in the grand princely law courts as one of the government's attempts to curtail monastic landowning (p. 126). If the peasants made their complaints at the

instigation of the government, then is it not odd that each of the fourteen complaints made against the monastery from 1480 to 1505 was decided in the monastery's favor? And if this period was the beginning of "the struggle for the secularization of church and monastic lands" as she entitles one subsection, then why can she cite only one clear case where the grand prince confiscated a village? And that option had been written into the original charter! (p. 112). Surely, something other than a wide-scale secularization offensive of the government is operative here.

DONALD OSTROWSKI
Harvard University

PAUL BUSHKOVITCH. *The Merchants of Moscow, 1580–1650*. New York: Cambridge University Press. 1980. Pp. xii, 212. \$16.95.

This compact study, an elaboration of the author's doctoral thesis, could be subtitled "a revisionist, quantitative analysis of some economic aspects of the Muscovite mercantile elite with special attention to its relationship to foreign and domestic trade." A preface, nine chapters, and a brief conclusion are supplemented by one print, two maps, two appendixes, notes, bibliography, and forty-seven tables (mainly statistical). The work is cautiously revisionist in challenging the generally accusatory historiography because of its allegedly abstract, debatable standards of comparison and its selective, largely descriptive use of fragmentary and heavily legalistic documentation to affirm the truism that Russian merchants were backward compared with their West European counterparts. Instead, Paul Bushkovitch rightly contends that early modern Russia should be studied in the context of its time and of the states of eastern and northern Europe. He therefore examines the richest group of Russian merchants in "their formative period" with particular attention to the tiny top stratum, the Moscow *gosti*, who received their privileges directly from the tsar and who numbered between twenty and thirty individuals during this period. Most of the book is devoted to detailed explication of the economic context in which the *gosti* functioned. Bushkovitch modifies the widely accepted thesis of great instability in *gosti* fortunes by showing that such was the norm everywhere except for some Dutch mercantile houses. From lack of sources, however, his study is largely "external" in using documentation—customs declarations, toll receipts, tax records, and legal materials—not generated directly by the merchants themselves. Indeed, one wonders how much record keeping the *gosti* actually did—the implication being that it was not substantial (if true, a significant fact in itself).

To supplement the sketchy printed sources, Bushkovitch employs some Soviet archival data (mainly customs records) and some Dutch and German manuscript materials on Russian exports and imports. He repeatedly affirms the importance of the Dutch trade via Archangel and envisages Muscovy as part of the far-flung Dutch commercial empire, downplaying the better-known Anglo-Russian trade. He concludes that the Moscow merchants played a large role in the dominant Archangel trade, an important role in the newly developing salt industry and trade, and a lesser role in other kinds of trade with other regions. Concerning the merchants' relationship to the state authorities, Bushkovitch argues against the traditional view of burdensome service and ruinous competition in favor of a mutually profitable interaction, especially as seen in their domination of toll collections.

This is an ambitious essay that, because of the breadth of the subject and the sparsity of previous scholarship, cannot resolve many of the problems it poses, as the author frankly acknowledges. Still, it leaves a general impression of disjointure and almost disembodiment concerning the *gosti* as people. Little general historical context is provided, and several books and unpublished dissertations were overlooked. Livelier prose and more careful editing could have improved the presentation considerably.

J. T. ALEXANDER
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J. L. BLACK. *Citizens for the Fatherland: Education, Educators, and Pedagogical Ideals in Eighteenth Century Russia*. (East European Monographs, number 53.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1979. Pp. 273. \$16.50.

This attractively produced little book does not give us a history of education in eighteenth-century Russia, as J. L. Black would be the first to agree. It deals summarily or not at all with the training of artists and architects, the education of clergy, private schooling, the early development of Moscow University, training in the crafts and trades, the press as an instrument of education, societies concerned wholly or in part with disseminating knowledge, the non-Russian schools of the Russian empire (in St. Petersburg or Moscow as well as in the Baltic provinces, the Ukraine, or elsewhere), and other such topics on which there are scholarly works to consult in Russian or in other languages. Instead, more than half of the book's 175 pages of text are taken up with four fairly discrete essays devoted to the main to the educational policies and interests of Empress Catherine II and particularly to the famous projects of I. I. Betskoi and Theodor Iankov-

ich de Mirievo. (In connection with the latter we are given an English translation by Elizabeth Gorky of the *Book on the Duties of Man and Citizen*, which was prescribed for use in the Russian state schools in 1786 and withdrawn, as somehow subversive, in 1819.) These richly detailed chapters are written with authority and verve, and on both general and particular points they might be usefully compared with Isabel de Madariaga's recent article, "The Foundation of the Russian Educational System by Catherine II" (*Slavonic and East European Review*, 57 [1979]: 369-95), which evidently appeared too late for Black to consider.

Black's other chapters, on education, loosely speaking, in pre-Catherinian Russia, have considerably less to recommend them. The non-Russian specialist interested in the spread of European education, to whom this book is also addressed, is warned that there are more questionable if not erroneous generalizations to be found in these chapters, more factual mistakes and senescent clichés, than one might have suspected or should have to endure. But to dwell on the insufficiencies of these earlier chapters would be to obscure the merit of those that follow and the usefulness of the entire book, with its extensive notes and bibliography, as a most handy work of reference.

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Chicago Circle

N. S. KINIAPINA et al. *Vostochnyi vopros vo vnesheinei politike Rossii, konets XVIII-nachalo XX v.* [The Eastern Question in Russian Foreign Policy from the Late Eighteenth to Early Twentieth Centuries]. Moscow: Nauka. 1978. Pp. 433. 2 r. 20 k.

A Soviet survey of Russian policy on the Eastern Question in the century and a half from the Treaty of Kuchuk Kainardji to the First World War that conveys factual information and treats "bourgeois" historiography in a fairly evenhanded manner may be praiseworthy in one context if not another. This cooperative effort by historians with different interests, headed by N. S. Kiniapina, gives Soviet students an outline that is generally accurate (despite some astounding errors—for example, that the Treaty of Unkiar Skelessi opened the straits to Russian warships) and an introduction to Western and tsarist as well as Soviet historiography. Studies by many Western scholars are listed in the bibliography and discussed along with Russian works in a historiographical survey that precedes the narrative. But the diverse nineteenth-century Russian literature on the Eastern Question and the extensive Western historiography critical of Russian policy are not well represented. A preference is shown for

the more patriotic prerevolutionary historians, for example, by the inclusion of the nationalist Tati-shchev, perhaps the true predecessor of Pokrovsky and Tarle, while the equally outstanding historian and professor of international law, Martens, is not mentioned.

Western students should find the book of value as an introduction to the Soviet interpretation. The Marxist-Leninist formula is clearly explained at the outset. The accompanying discussions of strategic and economic interests and of the popular and national issues that linked Russian, Balkan, and Near Eastern affairs are not unfounded. But the inferences that take the place of analysis are typically ideological, and nothing new of importance is introduced, despite the use of AVPR documents.

Serious difficulties arise in the attempt to present an "objective" history of the Eastern "Question." By avoiding the intellectual dimensions of the subject, the authors have put aside a tremendous legacy of Russian documentation and writings that reveals attitudes, perceptions, ideas, and doctrines informing decisions and setting guidelines for policy. For virtually the whole of conservative nationalist Russia the Eastern Question had a central place in explanations of Russia's past and mission vis-à-vis infidel East and heretical West. The grandiose notions expressed in romantic ideology appeared to be vindicated by the scope of real problems that the authors have also largely ignored.

The Eastern Question forcefully called to mind problematical relationships between Russia and the outside world. Whether or not it was solved after the revolution, it is the "question" that is missing from this otherwise useful but undistinguished survey.

HAROLD INGLE
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ANSLEY J. COALE *et al.* *Human Fertility in Russia since the Nineteenth Century*. Princeton: Princeton University Press. 1979. Pp. xxiii, 285. \$16.50.

The study of the decline of fertility since the late nineteenth century in the territory that now comprises the Soviet Union is an enormous undertaking. Ansley Coale, Barbara Anderson, and Erna Härm present a sophisticated statistical picture of the changes that have occurred in this culturally and economically heterogeneous area as part of the Princeton European Fertility Project. The Russian project, because of the tremendous diversity of the territory and the extreme difficulty of obtaining comparable data, is perhaps the most ambitious of the series. The study provides a look not only at demographic behavior but also an insight into the

strength of the ethnic traditions of the various population groups.

A decline in fertility is a common characteristic of all nations that are economically developed. This decline almost always follows a sharp drop in mortality, so modernizing areas generally experience rapid population growth over an extended period as mortality decreases earlier and faster than fertility. These processes at first produce a significantly younger population as the mortality declines are more pronounced at the younger ages. Only after an extended period of low fertility does the population's age structure become older.

Human Fertility in Russia provides an excellent test of the theory of demographic transition that, in greatly simplified form, contends that the impact of the industrial and technical revolution produces social and economic changes that result in both lower mortality and fertility. While lower mortality and fertility have generally occurred with development, their uneven nature among subnational groups presumably experiencing similar modernizing influences led to the need for a more systematic and sophisticated investigation of development and demographic change. The territory of the Soviet Union provides a perfect laboratory for such a study because of the extreme diversity of the population and the systematic and successful effort by a strong central government to impose those conditions—urbanization, industrialization, employment of women, education—that transition theorists postulated to be the cause of changes in demographic behavior.

The bulk of this study is a province by province statistical presentation of the changes in marital fertility, proportion married, age at marriage, and overall fertility from 1897 to 1970. In practically every case, the demographic behavior characteristic of modern society began in the Baltic provinces and spread to the east and south, first impacting the urban areas and then spreading to the rural ones. Variations in these indicators declined in all areas except the Central Asian republics in each census period; with this exclusion, indicators of demographic behavior in the Baltic, Slavic, and Transcaucasian areas, both urban and rural, were becoming more clustered.

The rates at which low levels of fertility were reached differed by nationality, with changes occurring more slowly in areas that were most different ethnically from the Baltic or West European populations. This lag in the reduction of fertility in some areas and the absolute increase in areas of Central Asia do not, according to the authors, invalidate the theory of demographic transition. In fact, the authors contend that "the decline of fertility in Russia in the twentieth-century confirms the broad generalizations about modernization and fertility pro-

posed in the theory of demographic transition" (p. 206). This study, with its meticulous presentation of data and painstaking analysis, emphasizes the extent to which cultural and attitudinal changes are prerequisite to demographic changes. While predicting that modernization and development will ultimately bring about changes in attitudes and thus demographic behavior in Central Asia, the authors emphasize the extent to which some nationalities are more resistant to such changes than others. Ethnic beliefs and attitudes, therefore, account for much of the variation found in demographic behavior within the overall framework of the theory of demographic transition.

JEFF CHINN

State University System of Minnesota

N. A. TROITSKII. *Bezumnstvo khrabrykh: Russkie revoliutsionery i karatel'naiia politika tsarizma, 1866-1882 gg.* [The Madness of the Brave: Russian Revolutionaries and the Penal Policy of Tsarism, 1866-82]. Moscow: Mysl'. 1978. Pp. 333. 80 k.

N. A. TROITSKII. *Tsarizm pod sudom progressivnoi obshchestvennosti, 1866-1895 gg.* [Tsarism before the Court of the Progressive Public, 1866-95]. Moscow: Mysl'. 1979. Pp. 348. 80 k.

These two volumes recount the political trials of the young Russian dissidents of the final third of the nineteenth century. These accounts of the court proceedings, domestic spying, administrative punishments, exile, prison terms, and executions are both stark tragedy and a grim lesson in how to exacerbate the alienation of an already disaffected youth. This critical subject has hitherto only partially been addressed by such scholars as M. K. Lemke, and these two volumes represent the first comprehensive treatment.

The author, N. A. Troitskii, very carefully logs each trial between 1866, when Karakozov made the first attempt on the life of Alexander II, and 1895. He finds that there were 226 separate political trials involving 1,342 individuals, of whom only 277 were acquitted. (As time passed, acquittals became increasingly rare.) Of those convicted, 137 received death sentences, although only forty-four were actually sent to the gallows. The overwhelming majority of the defendants were very young and well educated, very few being either older than thirty or illiterate. Indeed, this group in the fullest sense represented the flower of the youth of the nation. Their social composition, moreover, shifted from a predominance of gentry early in the period to reflect a more faithful cross section of the population in the 1880s and 1890s. Most were idealistic intellectuals who were adherents to *narodnichestvo*, the Russian

peasant socialism shaped by Herzen and Chernyshevskii, but with the beginnings of industrialization late in the century other socialists, including Marxists, displaced the populists.

Perhaps even more important than the trials themselves and the severity of the sentences was the prevailing climate of "establishment" opinion toward the alienated youth. Otherwise humane individuals reacted negatively and even irrationally toward the young. Long hair on a young man or short hair on a young woman constituted *prima facie* evidence of suspicious activity and warranted at least an investigation by the political police and condemnation by their elders. Probably typical in this respect was the noted diarist, A. V. Nikitenko, who expressed his extreme dismay at the sight of young women with bobbed hair and sunglasses (the latter were seen as a deliberate affront to authority) loitering about the medical academy seeking admittance to anatomical dissections. The government had no sympathy for these dissidents whose chief activity lay in propaganda and demonstrations directed against the very nature of one social and political system, and with the attack upon the emperor in 1866 it reacted with severity.

The post-1866 repression under Alexander II was augmented by the success of Count P. A. Shuvalov, the ambitious and clever chief of gendarmes, who played on the fears of Alexander II to produce a siege mentality that served to isolate the monarch even further from more moderate counsel. And it was in this period that the government, having just implemented a model judicial system, began to evade the procedural and jurisdictional guarantees mandated by those very reforms, and high officials, including the emperor himself, tampered with the justice system by seeking guaranteed convictions and death sentences. As the struggle reached a heightened intensity at the end of the seventies, the government began the use of military courts for political trials, a practice that became normal during the reign of Alexander III. By the end of the period of the study, the civil courts had been thoroughly subverted or bypassed to assure conviction and to "eliminate lawyer tricks," and provincial governors were granted extraordinary powers to treat suspected political offenders to summary justice. Individuals increasingly became even more subject to the capriciousness of officials, thereby compounding the alienation of the young people from the system.

The actions of the government in the long run were counterproductive. Feeling they had no protection in the law, the dissidents fought back. As they did so, the police increased their surveillance activities and young people increasingly were treated to arrest and summary exile, often without knowing either the nature of the charge or the duration of the sentence. Perhaps as many as thirty

thousand were shadowed by police spies at any one time, and this activity, far from being limited to the territories of the empire, reached into foreign states as well. In some cases the dissidents retaliated directly against their most visible tormentors—police officials, prosecutors, and provincial administrators—and then as both sides became more ruthless, they escalated their efforts until they succeeded in killing Alexander II on March 1, 1881. But others chose less violent means. Some, while in the docket, sought to impress the court and society with their dignity or high sense of purpose, and still others used their trials to make political statements intended for dissemination through the newspapers to society at large. Their courage and bearing did have an effect upon other Russian youth, and their lonely and courageous stands came to be admired abroad as well. In the end, the government managed to turn the disaffected and alienated youth into radicalized and desperate revolutionaries who progressively turned to more extreme purposes and methods. As Russia began to industrialize and to concentrate workers in the large cities, the mentally toughened and radicalized survivors of the imperial system of justice found fertile soil among the new urban classes and led them down the path of political action aimed at overthrowing the entire system rather than seeking limited social and economic concessions. One finds in this tragic story an important element in the conditions that brought about the fall of the old regime and an explanation of the character of some of those who ultimately succeeded to power.

One cannot help but compare the events of this story with the manner in which the imperial government's successors have dealt with dissidents and opponents. Horrible as the actions of the imperial government were, they never approached the comprehensiveness or pervasiveness that has characterized the system created by those who were schooled and hardened in the jails of the empire. Surely the frankness with which the author of these two volumes details the injustices of the old regime cannot be lost on thoughtful Russians who still may seek a government based upon the rule of law.

FORREST A. MILLER
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RICHARD PIPES. *Struve: Liberal on the Right, 1905–1944*. (Russian Research Center Studies, number 80.) Cambridge: Harvard University Press. 1980. Pp. xix, 526. \$32.50.

The second volume of Richard Pipes's biography of Peter Struve covers Struve's life and thought—mostly his voluminous thought—from his return to

Russia in October 1905 until his death in Paris in February 1944. These were Struve's lesser years—considering Pipes's judgment that he had his best ideas before he was thirty—yet more active and influential, and certainly more harrowing and tragic. Events went sour and made him more conservative, increasingly putting him out of touch with reality.

At first, in the thick of Kadet politics, he disagreed with Miliukov, preferring an opening to the right although drifting with the party majority until the debacle of 1906–07. Concluding that Russia lacked the prerequisite cultural foundations for constitutionalism and warning that the monarchy could not survive another round of war and revolution, he then voiced nationalist and imperialist views, talked with Stolypin, and turned against the intelligentsia. His contribution to *Vekhi*, Pipes notes, was not among his best, but his arguments were on target: the Russian intelligentsia—negative, doctrinaire, opportunist, and anarchist—feared political responsibility. Among his new allies, who after 1911 called themselves the Progressive party, Struve advocated freedom, culture, and a vigorous foreign policy. In August 1914, patriotically forgetting his previous warning, he looked forward to a more glorious Russia fulfilling its imperial task and Slav mission. After playing a prominent part in the administration of economic warfare, he was sidelined by the February Revolution. The year 1917, Pipes states, found him "mainly preoccupied with writing." His influence over the course of events in Russia came to an end with a futile speech at the pre-parliament.

His subsequent career among the White forces and in the emigration Pipes likewise describes in meticulous and compassionate detail. The section on emigration politics and schools of thought offers an admirable exposition of this little-known subject. Of equal distinction and honesty is his chapter on Struve's explanations of "What Happened" in 1917. Uncertain, contradictory, shallow, and unconvincing, these explanations show the limitations of Struve's mind. He never asked himself how, amidst the available human raw material, the liberal individualism for which he stood had any chance nor, more importantly, how, in an age of mass politics, a Russian state capable of facing the competition of *Weltpolitik* could be constructed.

Essentially Pipes's Struve biography is a fine work of scholarly filio piety. In its completeness and sincerity it provides all the essentials for a future hardheaded and critical assessment. Such assessment in the full contexts of Russian history will prove no doubt that Struve was an obstinate, doctrinaire, intolerant *intelligent* of the old school who, although he impressed many contemporaries, did nothing right and carried nothing to a proper conclusion and who, true to type, yet was also a lovable and worthy human being. One wonders, though,

whether even on that score he quite deserves the distinction that Pipes has bestowed upon him.

THEODORE H. VON LAUE
Clark University

CHRISTOPHER READ. *Religion, Revolution, and the Russian Intelligentsia, 1900-1912: The Vekhi Debate and its Intellectual Background.* Totowa, N.J.: Barnes and Noble. 1979. Pp. ix, 221. \$25.00.

In a world grown tired of words, at a time when so much thought is debased into mindless ideology, our continued fascination with the impassioned debates of the Russian intelligentsia calls for explanation. Perhaps nostalgia overcomes us, and we seek renewal by reliving discussions where every idea counted and the smallest point of difference was debated as if the world depended on it. Or perhaps, as Christopher Read suggests in this analysis of the Russian intelligentsia in the early twentieth century, we study these debates because they considered questions of more than parochial interest and came to conclusions that remain live options among Russian dissidents today.

The periods immediately before and after the abortive Russian Revolution of 1905 witnessed especially lively debates among the intelligentsia. A rich and varied legacy had been inherited from the preceding century; slavophilism, liberal Westernism, nihilism, populism, and Marxism—all continued to inspire intolerant partisans. But new positions emerged as well. Although few of the intelligentsia took part in what Nicholas Zernov has called the twentieth-century "religious renaissance," many of them wrote on questions traditionally defined as religious and previously taboo among the intelligentsia. Moreover, prodded by the futile, yet threatening, experiences of violence in 1905 and aware of new opportunities to achieve positive change within the system, some among the intelligentsia came to a re-evaluation of the materialism, socialism, and hopes for revolution that had long been the common currency of the Russian radicals.

In Read's view, the dialectical interaction of the idealist and materialist schools and challenges to collectivism founded in concern for freedom of the individual might have brought forth from the intelligentsia yet another major school, that of *liberal idealism*. As it turned out, however, the symposium *Vekhi* (*Signposts*) of 1909, in which many of the new ideas were expressed, evoked no more than negative criticism and misunderstanding, leaving hope only in a tenuous legacy for later generations.

Read's most valuable contribution is the meticulous reconstruction of the series of arguments that troubled the intelligentsia for over a decade, from

1900 to 1912. He defines and catalogues the issues at the heart of the debates, he summarizes the positions taken in a large number of works that even most historians of Russia will probably never have time to read, and more importantly, he explains how some pieces built on others or were written in opposition to others. Thus Read provides a context in which all the arguments are better understood. He also provides an excellent bibliography and endnotes for readers wanting to go further.

Unfortunately, Read sometimes seems all too close to his material and thereby loses the perspective of relative importance and appropriate pacing in his exposition. In addition, one can complain that the type of brief, insightful sketch he gives of Nicholas Berdyaev as a person and a writer appears too rarely in his study. But these are small objections to a needed and welcome book.

WILLIAM F. WOEHRLIN
Carleton College

RONALD SEGAL. *Leon Trotsky: A Biography.* New York: Pantheon Books. 1979. Pp. 445. \$15.00.

ROBERT WISTRICH. *Trotsky: Fate of a Revolutionary.* London: Robson Books; distributed by Rowman and Littlefield, Totowa, N.J. 1979. Pp. 235. \$19.00.

The Trotsky mystique has lost surprisingly little of its vitality in the forty years since his death at the hands of a Stalinist assassin. But, paradoxically, the Trotskyist legacy has languished: the original ideology is ambiguous or simply irrelevant, the political movement anemic and exotically sectarian. In the Soviet Union the old demonology, a noxious residue of primitive Stalinism, lingers on. Serious consideration of Trotsky, as distinct from polemics, was virtually unknown during his lifetime, and after his death his fascinating career lay almost fallow, historically speaking, until the publication in 1954-63 of Issac Deutscher's monumental trilogy. An accomplished journalist turned scholar, Deutscher wrote with verve and pace, buttressing his findings with impressive though not exhaustive research. If by no means a definitive work, it apparently inhibited others who might have helped fill the biographical gap. There were, however, rumblings even at the time protesting that Deutscher, an independent Marxist, was entirely too sympathetic to his protagonist to deal evenhandedly with so controversial a figure.

In recent years, as the Deutscher interpretation lost some of its luster (partly, perhaps, because neither the author nor his subject proved to be the best of prophets), there has been a modest resurgence of Trotsky studies. Among others, Richard B. Day has examined his economic policies, Joseph Nedava has

probed his Jewish heritage, and Baruch Knei-Paz has dealt at length with his ideas and theories. The two books under review add further perspective to a continuing re-examination of a remarkable but flawed revolutionary hero still without honor in his homeland. Ronald Segal has written a conventional "life and times" biography that as a literary effort is eminently readable, even distinguished, provided one's taste runs to a heavy dose of metaphor. Its merit as history is less assured. Robert Wistrich's slender volume is essentially a study of Trotsky as an ideologist and political tactician, with only minimal attention to biographical detail, especially after 1917. Neither work is based on fresh research or, for that matter, a thorough perusal of the traditional sources (the Trotsky Archive at Harvard, for example, was unexamined). But Wistrich, although his aim is less ambitious, demonstrates greater knowledge of the available literature, including that in Russian. Segal's shallow bibliography lists no works in a foreign language, but there are a few reference notes to Russian sources. Both authors, as do most of us who have ventured into the field, bear an intellectual debt to Deutscher, and Segal, who has the most reason for gratitude, has generously acknowledged it.

Nearly all of the Segal biography is a sophisticated apologia for Trotsky. Only toward the last quarter of his narrative does the author develop a mildly critical spirit, although there are previous remarks that could be construed as unfavorable. One would never guess from this account that Trotsky was a woefully inept politician or, despite a few gingerly made references to his mental health, that his numerous psychosomatic complaints were a severe handicap at times of political stress. Only by implication can the reader know of Trotsky's prickly vanity and arrogant personality or that his reputation was hardly that of a convinced democrat fighting on principle against the growing despotism of Stalin's party "machine." Because Segal has no obvious ideological commitment other than his unduly benign view of Trotsky, his reluctance to deal with such fundamental issues is regrettable. Specialists will have little reason to consult this work, but the educated public should enjoy it as a high-level popularization that is reasonably accurate as to questions of fact and less demanding than Deutscher's three volumes.

Wistrich's book is clearly the better of the two. It requires a greater intellectual effort, for the exposition is presented in an analytic framework, and the style, if pleasing, is less elegant than Segal's. It tends to assume too much knowledge on the reader's part—presuming a general audience—but this may be inevitable in such a highly compressed work. The approach is basically chronological, with such topical deviations as Trotsky's celebrated theory of

permanent revolution, his literary career, and the concept of a Soviet "Thermidor" (each is assigned a chapter heading). The author displays no partisanship but does emphasize Trotsky's blunders and weaknesses to a greater extent than previous writers, with the notable exception of Joel Carmichael's rather dyspeptic treatment. This version is certainly a stimulating corrective to the Deutscher-Segal "school" while avoiding the shrill tone of zealous anti-Trotskyism. The closest approximation to this compendium is Irving Howe's *Leon Trotsky* (1978)—possibly a more lucid appraisal of Trotsky's ideas, but at the same time, as Wistrich suggests, less successful in avoiding the seductive spell of his "flamboyant rhetoric and grandiose theories" (p. 7). This is an unassuming but welcome addition to the growing literature on Trotsky.

ROBERT D. WARTH
University of Kentucky

ROBERT C. WILLIAMS. *Russian Art and American Money, 1900–1940*. Cambridge: Harvard University Press. 1980. Pp. vi, 309. \$17.50.

This study combines history, spy thriller, and mystery novel in one volume. Robert C. Williams merges art history with diplomatic history as he contends that Russian art reached America because the Russians sought political, economic, and diplomatic advantages through either its display or sale in the United States. The paintings sent to St. Louis in 1904 failed to win American sympathy and disappeared through a complicated bit of chicanery involving several Russian and American "entrepreneurs." During the early five-year plans the Soviets tried to fill their coffers by arranging sales abroad of confiscated private *objets d'art* and paintings; they even raided the fabulous Hermitage collection when the price was right and the need pressing.

Russian art exhibits in America for propaganda purposes were disappointing to the Soviets, but, when it came to sales of the "excess baggage of bourgeois culture," they scored heavily, generating over \$20 million between 1928 and 1933 alone. The success was greatest when sales were arranged privately and, sometimes, secretly. At this juncture the names of presidents, cabinet members, art dealers, adventurers, and "characters" weave through the narrative, tying these people to schemes ranging from naive to nefarious in a bewildering succession of adventures and misadventures. Certainly Andrew Mellon's involvement was both nefarious and a misadventure, although he profited from it. His often-denied secret purchase of Hermitage paintings got him in trouble with the Internal Revenue Service and played some part in the efforts to im-

peach him initiated by Congressman Wright Patman.

Sometimes the author reaches too far for the influence of Soviet art in America. For example, "Whether the [Armand] Hammer sales [of Russian art in America] changed anyone's opinion in favor of recognizing the Soviet government one cannot say" (p. 224). One *can* say that it was not significant in determining the opinion of the one who counted most—FDR—who acted for reasons of security above anything else.

Williams is on firmer ground when he relates the influence of official Soviet assistance on Joseph and Marjorie Post Davies when they were aided in obtaining a fortune in Russian art and curios while Davies was U.S. ambassador to the USSR. Their pro-Soviet sympathies were reinforced by these "kindnesses" and by their incredible naiveté.

There are some weaknesses in the book—primarily the repetition of the thesis, which appears in the introduction, several times in the body, and again in the conclusion. This is an unnecessary pedagogical device for readers who will be attracted to this study. Also, on page 227, the author credits Armand Hammer with the suggestion in 1940 for the lend-lease exchange of British Caribbean bases for American destroyers, and there is no footnote for this interesting bit of information. This happens at least two other times.

This is a very significant contribution to the history of Russian-American relations. It is exhaustively researched in primary sources, well written, and provides ample evidence that, from the outset of the Soviet period, ideologues in both countries would compromise their "principles" if they wanted something badly enough from the other side.

EDWARD M. BENNETT
Washington State University

RENATA FRITSCH-BOURNAZEL. *Die Sowjetunion und die deutsche Teilung: Die sowjetische Deutschlandpolitik, 1945–1979*. Foreword by ALFRED GROSSER. Opladen: Westdeutscher Verlag. 1979. Pp. 167.

Helmut Schmidt's latest visit to Moscow has focused attention on the importance of the Soviet-German connection. Not only have Schmidt and Leonid Brezhnev discussed such major East-West questions as armaments and Afghanistan, but Soviet-West German bilateral relations have grown in significance. The Federal Republic is the USSR's leading non-Communist trading partner and a major supplier of technology; West Germany increasingly fills its energy needs with Soviet imports. Moreover, the strength of the Bundeswehr and Bonn's advocacy of "Eurostrategic" missiles have raised the military significance of West Germany for the USSR.

All this may seem to be the familiar, major themes of European diplomacy. Yet events since 1945 have put these relations in quite another light. We think of Germany as defeated and divided, with each half a dependent member of opposing alliances; the Soviet Union seems to hold all the trumps in dealing with Bonn and to have achieved a long-standing goal of dividing Germany.

One turns to this detailed, capably written study by Renata Fritsch-Bournazel for insight into how and why German-Soviet relations came to have their present shape and what their future may be. Does it provide such understanding?

As a narrative of Soviet-West German relations, basically from the West German perspective, this is a useful work. In almost half the text, it takes us from Potsdam to the mid-1960s. Subsequent chapters show how both Bonn and Moscow had diplomatic and domestic needs best served by new policies and how, in a burst of creative diplomacy in 1969–73, the present middle-European status quo was created. The book ends with a superficial survey of the European and bilateral consequences of this new situation.

There are several weaknesses. The *triangular* nature of Soviet policy in Germany is left unclear by the author's neglect of the GDR's role and its impact on Soviet policy. The focus on diplomatic history slights the impact of domestic politics on international questions. The importance of Bonn's Western ties in shaping its policy toward Moscow is not carried forward from the Adenauer era to the present.

* Finally, would that the author had let her conclusions show! Was a genuine opportunity for German unification missed in 1951–52? Did the Soviet Union after 1968 shift to the Federal Republic as its chief European partner? Could Bonn have gotten more out of the 1970–72 negotiations?

Such inadequacies leave this book just another acceptable account of a fascinating, important transformation.

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NEAR EAST

JACOB LASSNER. *The Shaping of 'Abbasid Rule*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1980. Pp. xvii, 328. \$25.00.

One of the great dates in world history is 750 A.D. when the Abbasid dynasty replaced the Umayyads in the caliphate and transferred the Islamic capital from Syria to Iraq, where it remained for the next five centuries. The Abbasid empire reached a peak of power and influence early in its history, traditionally identified with the reign of Hārūn ar-

Rashīd (786–809); this was followed by a prolonged decline and fall. Jacob Lassner's study is focused on its successful early years, particularly the caliphate of Hārūn ar-Rashīd's grandfather, Abu Ja'far al-Manṣūr (756–775), the founder of the imperial city of Baghdad.

As Lassner observes, the sources for those momentous years date from later centuries and are sketchy and untrustworthy. He has had to rely on "medieval chronicles and belles-lettres" of a "highly tendentious character" (p. xv). His book, therefore, makes no claim to be a definitive full treatment of early Abbasid imperial history. Rather, it is a series of linked but independent essays on particular topics, arranged in two groups. Five of the essays treat the "political setting" and three the "physical setting" of al-Manṣūr's eventful career. These are supplemented by a preface, introduction, and postscript, and by genealogical charts, a few maps and ground plans, fifty pages of notes, and an extensive bibliography. The reader would appreciate more detailed maps and a glossary of the many Arabic terms employed by the author.

By starting each of the eight chapters with a quotation from the sources, the author introduces us to a society very different from our own and to the difficulties encountered by the scholar in appraising the quotients of authentic information and special pleading in each source. The material on the political setting offers many insights into the tangled and sometimes ruthless relationships within the large Abbasid family. It also evaluates the extent to which the early caliphs employed both military men and civilians in governing their huge empire. Already in al-Manṣūr's day the seeds were planted for policies that would lead to the Arab caliphs' dependence on Turkish soldiers and Persian administrators without, however, decreasing the destructive rivalries within the Abbasid family. The endemic weakness of their caliphate seems to have been incurable virtually from its beginnings.

The chapters on the physical setting, which also include a large component of politics, discuss the reasons for the transfer of the capital from the region of Kufa, on the Euphrates, to a new site at Baghdad, on the Tigris. Kufa, which had been the capital of Ali, the fourth caliph, continued to be a center of discontent and intrigue for the Shi'ite Muslims who considered the Sunni Abbasids as usurpers depriving Ali's descendants of their proper position as Muslim rulers. Lassner supplies many fascinating details about al-Manṣūr's famous "Round City" at Baghdad, a huge fortified complex of palace, mosque, and government offices and residences. Difficulties with maintaining adequate security and providing water and food for the thousands of residents obliged al-Manṣūr to move to more conventional quarters outside the Round City. Of equal interest is the discussion of the maze

of canals, markets, workshops, barracks, palaces, and other dwellings that, together with the Round City, comprised Baghdad.

Students of Abbasid history will welcome this volume and look forward to the companion historiographical study Lassner plans in the future.

JOHN B. CHRISTOPHER
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RICHARD W. BULLIET. *Conversion to Islam in the Medieval Period: An Essay in Quantitative History*. Cambridge: Harvard University Press. 1979. Pp. 158. \$15.00.

This book is an effort to gauge the rhythm of conversion of Middle Eastern peoples in the early centuries of the Islamic era. Until recently it had been assumed that the majority of Middle Eastern peoples had accepted Islam in the wake of the Arab conquests in the seventh century. Now, on the basis of research in literary sources, it is clear that in many provinces substantial conversion from Christianity, Zoroastrianism, and Judaism was delayed many centuries.

Richard W. Bulliet's contribution to this subject is a novel technique. By examining the names and genealogies in medieval Iranian dictionaries (for Nishapur and Isfahan) of prominent Muslim scholars and saints, he is able in some cases to identify the first family member to convert to Islam and to calculate the approximate time of the conversion. By tabulating the popularity of certain Muslim names he is able to graph the progress of conversion from generation to generation. His method is based on numerous assumptions and approximations, most of which are acceptable for a rough graph of conversions. One assumption, totally unexamined and not entirely plausible, is that the dictionaries actually represent a sample of the total population and that one can deduce the rate of conversion in Iran as a whole rather than that of the particular stratum of population represented in the dictionaries. Bulliet's results place the great wave of conversion in the late eighth and ninth century; he concludes that conversion was substantially (90 percent) complete by the end of the ninth century. In my judgment, numerous Iranian villages and districts remained Zoroastrian well into the tenth century, if not later.

The use of the data on Iran is a very clever way of teasing new information out of unyielding sources, but Bulliet then tries to extrapolate the results in Iran to the provinces of the Arab world, where the data do not permit the identification of first-generation converts by name. This seems an unnecessary exercise, since the tissue of assumptions is now stretched too far for any historically reliable conclusions.

Further, Bulliet goes on to interpret all of Islamic

history in light of his conclusions about the rhythm of conversions. Although conversion and rate of conversion are crucial factors in understanding social history, this seems a little extreme. For example, he several times attributes movements for provincial separation from the caliphate to the presence of a self-confident Muslim population that no longer needed caliphal protection from a non-Muslim majority. This leaves out of account a host of equally relevant considerations such as the rise of slave military forces, the decline of caliphal bureaucracy, and the shattered legitimacy of the Abbasid regime in the ninth century. What needs to be done is to integrate the several considerations that affect the large features of Muslim history rather than to assume the priority of the conversion factor.

Bulliet has also put the methodological cart before the horse. He interprets known history by the new finding, but the known history is not problematic; it is the new assumed rhythm of conversion that needs to be supported by other historical data to demonstrate the plausibility of the method. Unfortunately, Bulliet simply assumes the validity of the results and does nothing to muster corroborating, or opposing, information. Having over-committed himself to his method, Bulliet has accepted tentative results as proven fact and has erected an interpretation of Islamic history upon the conclusions. The speculative interpretations make the initial insight and method less, rather than more, persuasive.

Conversion to Islam is done with the special imagination, innovativeness, and eye for unusual opportunities that Bulliet has put to such good use in all his work. With more caution, the technique no doubt has a part to play in supplementing our all-too-scanty evidence on a crucial issue.

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GALAL H. EL-NAHAL. *The Judicial Administration of Ottoman Egypt in the Seventeenth Century*. (Studies in Middle Eastern History, number 4.) Minneapolis: Bibliotheca Islamica. 1979. Pp. ix, 109. \$16.00.

There have been a number of scholarly works based on an examination of materials in the Ottoman archives in Istanbul and Ankara as well as an increasing number from the empire's major provincial cities in Rumeli and in the Arab provinces. This is especially true since World War II, when the archives began to be more accessible to Western scholars and, indeed, utilized in a systematic fashion by indigenous scholars as well. Most of the initial works from this period concerned the land tenure systems and tax procedures instituted by the

Ottomans after their conquests and are recorded in the *mufassal tahrirler*, *ısmal tahrirler*, and *tapu defterler* (land and tax registers). One of the classic works of this genre is that of Halil Inalcik's study of Albania published in 1954. It is worthy to note that Inalcik, now professor of Ottoman and Islamic history at the University of Chicago, is acknowledged to be the main inspiration of the work under review even though the topic is now that of Islamic law in seventeenth-century Ottoman Egypt.

There is an important revisionist thread, at least as far as Western historiography is concerned, in most of Inalcik's and his students' work. Briefly put, their studies demonstrate the variegated, spontaneous, and self-generative nature of many Ottoman institutions (towns, guilds, and legal institutions) and to a large extent that of their Islamic and non-Islamic precursors as well. The literary and theoretical works upon which earlier studies were largely based, and from which many books were written in the latter nineteenth century and in the first six decades of the twentieth century, depict a much more unspontaneous, rigid, and monolithic culture than archival records seem to indicate. This is true even when one acknowledges that the preserved and usable archival materials are sometimes later in date than the literary sources.

Galal H. El-Nahal's study of the *Shari'ah* (religious) courts of seventeenth-century Cairo supports the archival view of Ottoman Islamic society. This view is contrary to late nineteenth-century scholarship and the prevailing scholarship of the twentieth century concerning the *Shari'ah* (Islamic sacred law) and the *fiqh* (Islamic jurisprudence), which maintain there was little connection between theory and practice and that the *Shari'ah*, as far as applicability was concerned, was simply a normative dictum best illustrated by the expression that the *Shari'ah* was "an ideal doctrine for an ideal society." El-Nahal, in five short chapters (pp. 25-71), gives excerpts extracted from criminal, civil, family, social, and *aw-qāf* law from the *Shari'ah* courts and comes to the conclusions that the *Shari'ah*, in the courts where it was applicable, was not an ideal doctrine for an ideal society but, indeed, a doctrine for a dynamic, living, and real society. Given the limitations of his data and his research, El-Nahal seems to establish his point.

The Islamic world has been viewed by the West and Western "orientalists" principally through the too static views of the "religious" interpretation of the Islamic literary and theoretical material. It is fitting that this view should be contested with the proposition advanced by El-Nahal that the harmony between the legal expressions of religion in the *Shari'ah* and social organizations was at once greater and less than the literary expression "ideal doctrine for an ideal society" implies.

El-Nahal's essay is short, and I think the needs of the scholarly community could have been met more inexpensively if it had been published as an article in an appropriate journal.

ROBERT OLSON
University of Kentucky

JOHN R. PERRY. *Karim Khan Zand: A History of Iran, 1747-1779*. (Publications of the Center for Middle Eastern Studies, number 12.) Chicago: University of Chicago Press. 1979. Pp. xi, 340. \$12.50.

Although this work is primarily a study of the life of the founder of the Zand dynasty, it also fills a gap in our understanding of an obscure period that was a background to the Qajar era and the history of modern Iran. The first two sections of the book are concerned with the rise to power of Karim Khan and the establishment of his rule, while the last part is concerned with his administration, foreign relations, and the society that was in part formed by the actions of Karim Khan. The first two parts, essentially a political and military history of the time, are well written and informative, a valuable contribution to the chronology of events. The last section is really a continuation of the same style rather than a detailed analysis of the institutions of government and administration under the Zand dynasty. As a summary of internal conditions, however, it is commendable. John R. Perry himself admits that it is not possible to write a social and economic history of the Zand period in isolation from the entire period ca. 1600-1800, and all he wishes to do in this book is to give a "presentation of materials that may help to signpost trends" (p. 223).

Although the author has worked hard to present an understandable narrative of events, two points arise. First, a reader needs considerable background to follow the intricacies of court intrigues and the complexities of tribal and family factions in Iran at this time. Second, the author sometimes neglects to tell us why certain events occurred or at least why changes in policy came to pass. For example, we hear that "word was already abroad of Karim Khan's vengeful approach" (p. 29). Presumably this means his revolt against 'Ali Mardan. The background or reason for "vengeance," however, is not explained. Furthermore, the use of many village names without definite geographical locations assigned to them sometimes confuses the narrative.

Perry attributes the success of Karim Khan not only to his magnanimity toward defeated opponents and his wide popular support based on his reputation for humility and lack of ostentation (in marked contrast to his predecessor, Nader Shah, and his successor, Fath Ali Shah Qajar) but also to his successful combination of tribal and urban forces

in Iran. The period of Nader Shah and Karim Khan was one of the resurgence of the tribes and the disintegration of central authority. Yet Karim Khan was able to create a kind of balance between the tribal chiefs and the urban leaders by appointing the chiefs as governors or local tax gatherers, thus ensuring a community of interests and not conflict. But the power of the tribal chiefs increased, and this pattern continued into the Qajar period, for both the Zands and the Qajars, after all, were tribes. Under Karim Khan, however, the monopoly on power held by Turks, whether tribal or settled, was broken, never to be secured by them again. Although Karim Khan did not change the government or the pattern of rule in Iran, by his appointments and by his mildness and conciliatory policies toward enemies he brought a period of economic stability and progress such that the later dynasts looked back to the reign of Karim Khan as a kind of "golden age" in the turbulent modern history of Iran. Certainly the citizens of Shiraz, his capital, have not forgotten him, and recent attempts to erase all memory of the monarchical past in Iran have not changed the names of Karim Khan and Lutf 'Ali Khan Zand on the two main streets of the city.

This work will be the authoritative reference work on Karim Khan Zand, and its diligent use of Persian, Russian, and Western sources has assured the reader that it will not soon be displaced.

EDEN NABY
Harvard University

NORMAN A. STILLMAN. *The Jews of Arab Lands: A History and Source Book*. Philadelphia: Jewish Publication Society of America. 1979. Pp. xx, 473. \$14.95.

The subject of this book is one that has in recent years been a magnet for propagandists of all sorts. On the one hand, there are those who portray the Arab-Jewish relationship in excessively roseate hues as one of intercommunal harmony, racial toleration, and cultural symbiosis. On the other hand, there has arisen more recently a revisionist interpretation that stresses the elements of violence, persecution, and discrimination in the Jewish experience in Arab lands. The first view, as Bernard Lewis pointed out some years ago, was largely the invention of the first generation of European orientalists, mostly Jews, who used the myth of the supposed Arab-Jewish golden age as a stick with which to beat their own imperfectly tolerant societies. Oddly enough, in spite of the current radical chic vogue (particularly in the U.S.) of denouncing all orientalist scholarship as part of an imperialist conspiracy, this myth is almost the only bit of traditional orientalism that the anti-orientalists have swallowed whole—perhaps it is not a coincidence that these

latter should find the myth of the golden age of Arab-Jewish brotherhood extraordinarily useful in their propaganda against Zionism. By contrast, some Zionists have been in the forefront of the revisionist school, emphasizing the less attractive features of Arab treatment of Jews. The popularity of this approach in some quarters cannot wholly be disconnected from its usefulness in pro-Zionist propaganda.

Norman A. Stillman explicitly eschews both of these approaches. His book consists of an introductory narrative of 108 pages, followed by over 300 pages of selected historical documents. The first section is an admirable synthesis of the current state of scholarship, although, inevitably, given the scope of the work, some themes (especially in the modern period) are treated rather sketchily. The documents have been chosen with meticulous care to illustrate the various facets, periods, and areas concerned; many are here published for the first time in English. Throughout, the author demonstrates a scrupulous sense of balance and objectivity. The picture that emerges is mixed, although the dominant shade of grey steadily darkens as we move into the modern period (more fully dealt with in the documents than in the introduction). The ambivalence of the Muslim-Jewish relationship emerges early: in *sura* 9:29 of the Koran, Muslims are enjoined to fight against Jews and Christians "until they pay the *jizya* [a special tax levied on non-Muslims] out of hand, and have been humbled." On the other hand, at the time of the Muslim conquest of Palestine, we find Jews in Hebron in 638 A.D. collaborating with the Arabs in the capture of the city (a poignant reflection on more recent events there). After the period of relative toleration in Muslim Spain, the picture blackens: Jews in Fez from the early sixteenth century onwards are forced to wear sandals of straw; Muslims in Jerusalem about the same time, it is reported, beat Jews on their moustaches with shoes; the blood libels against Jews in Syria and Egypt in the mid-nineteenth century are of a piece with the predominant modern attitude that culminated in the departure of nearly all Jews from Arab countries.

The elegant production of this book is spoilt only by the inadequate quality of the pictorial reproductions, one of which, the picture of the Geniza fragment opposite page 90, is unfortunately printed up side down.

BERNARD WASSERSTEIN
University of Sheffield

AFRICA

NEWELL M. STULTZ. *Transkei's Half Loaf: Race Separatism in South Africa*. New Haven: Yale University Press. 1979. Pp. xv, 183. \$16.00.

Twelve years after *South Africa's Transkei: The Politics of Domestic Colonialism*, which he wrote with Gwendolen M. Carter and Thomas Karis, Newell M. Stultz returns to the subject in a solo performance explicitly designed to assess whether South Africa's first independent Bantustan has any potential for reducing racial conflict within southern Africa. Characterizing his orientation as "conservative, or at least on the conservative side of center" (p. xiv), Stultz addresses two groups: students of multi-ethnic societies and potentially influential, white, Afrikaans-speaking South Africans close to the centers of political power. His closely reasoned argument should be taken seriously by both of his target groups as well as by anyone with an interest in the nature of separate development and its consequences for race relations in South Africa.

Explicitly eschewing the approach of a political historian, Stultz sets out a framework for analysis in an opening chapter through an ordered examination of separatist ideas, including both partition and federalism, that have been put forth by some dozen authors outside of official government circles or those closely linked to them. In three subsequent chapters, he rigorously examines the extent to which Transkei independence provided "a reasonable and practicable division of the resources of South Africa" (p. 17), the degree to which the decision to take independence represented a popular mandate by Transkeians, and a calculation of who has benefited from Transkei independence. He then analyzes the events of the first year of independence before turning in his final two chapters to his estimate of whether Transkei independence has been a "liberating" step toward eliminating racial discrimination and oppression.

The title of the book suggests the tenor of Stultz's final assessment. He concludes that Transkei independence falls "far short of what a partition solution would seem to require" (p. 137); from the perspective of a decolonization process designed to achieve more harmonious racial relations in South Africa, it also fails. He finds "Transkei independence might in time prove to be an important, if not direct, step to eventual power-sharing in the region through federation" (p. 152), although he also asserts that it is clearly retrogressive in its unequal distribution of the collective wealth of South African society and its unfair denial of South African citizenship to urban Xhosa. In terms of his underlying concern for conflict resolution, Stultz finally finds that Transkei independence "is largely irrelevant to the political tensions of southern Africa primarily because it does not honestly address these tensions where they are most evident, among blacks in the cities" (p. 160). Nevertheless, Stultz does sympathize with the view that Transkeians resident in the territory have gained some advantages by taking one of the "exceedingly limited" options open to

them "to purge apartheid from their own lives" (p. 167).

SHERIDAN JOHNS
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ASIA AND THE EAST

CHI-YÜN CH'EN. *Hsün Yüeh and the Mind of Late Han China: A Translation of the Shen-Chien with Introduction and Annotations*. (Princeton Library of Asian Translations.) Princeton: Princeton University Press. 1980. Pp. ix, 225. \$16.00.

This book presents a systematic introduction to the philosophy of Hsün Yüeh, an important Confucian thinker of the late Eastern Han period, and an annotated translation of the shorter of his two surviving works. It complements an earlier study by Ch'i-yün Ch'en of the life and thought of Hsün Yüeh as a reflection of the turbulent times in which he lived.

Hsün Yüeh's thought reflected the philosophical syncretism characteristic of the Han. He wrestled with the familiar Confucian problems of the relationships between self-knowledge and knowledge of the external world and between knowledge and action and with the problem of achieving self-mastery. His approach to the problems of good government and social order was sympathetic to the position of Confucian idealism, but he was not afraid to recommend expedient solutions more appropriate to an age of pervasive disorder.

The book is nearly equally divided between an introduction and the translation of Hsün's *Shen-chien* ("Extended Reflections"). The translation of the treatise itself (which comprises five chapters: "The Essence of Government," "Current Affairs," "Common Superstitions," and two sets of "Miscellaneous Dialogues") is accurate and effective; one might quarrel with an occasional word or phrase while relying on the whole. The extensive annotation traces literary allusions, elucidates difficult or corrupted passages, and, in general, provides all the help one might wish.

The introduction is divided into three chapters. Setting aside the first for a moment, one may note that in the second chapter, Ch'en provides a comprehensive study of the textual history of the *Shen-chien* and of Hsün's much longer work, the *Han chi*; he concludes that the texts as we now have them, though corrupted and incomplete, are the authentic work of Hsün Yüeh. The third chapter presents translations of a few chapters of the *Han chi* that complement the *Shen-chien* and aid our understanding of it.

The opening chapter of the introduction examines the philosophical background of Hsün's thought, and provides a summary and discussion of his philosophical position. The latter is well done.

The author points out that the main problem of philosophy, as Hsün saw it, was "the incomplete and imperfect state of man's knowledge" (p. 37); thus Hsün tried to steer a middle course between the moral dogmatism and the philosophical skepticism that he had received as the heritage of earlier thinkers.

It is only in the introductory section on philosophical background that Ch'en falls short. He in effect tries to summarize all of Chinese philosophy from Confucius to Hsün Yüeh in under thirty pages. The result is predictably sketchy, and it is further marred by an attempt to justify Hsün's predicament—dogmatism versus skepticism—by dealing with all earlier Chinese thought in those terms. Important and complex thinkers are thus neatly pigeon-holed in categories that range from the inappropriate (Confucius and Chuang-tzu as "agnostic skeptics") to the seriously misleading (Tsou Yen as a "positivist"), and, in the process, thinkers who have little in common save these imposed characterizations (Tsou Yen and Han Fei, for example) are lumped together to make very strange bedfellows.

Nevertheless, with the exception of these opening pages, the book is very well done; students of early China will welcome this useful presentation of the work of an important philosopher of the later Han, an age about which too little still is known.

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RICHARD P. SUTTMEIER. *Science, Technology and China's Drive for Modernization*. (Hoover International Studies, number 223.) Stanford, Calif.: Hoover Institution Press. 1980. Pp. xii, 121. Paper \$6.95.

China's much-publicized current drive for scientific and technological development is intimately linked with fundamental post-Mao politics, as well as with a desire for increased productivity, and with emergent alignments in international relations. These assumptions inform this thorough, lucid, and cogent analysis. Richard P. Suttmeier's book is the best available introduction to the institutional structures, political processes, and ideological conflicts that are central to China's recent commitment to the "four modernizations." The author is particularly good at describing and explaining the administrative organization of China's aborning research and development establishment. His familiarity with the entire history of the People's Republic makes it possible for him to place into historical context recent changes in the educational system or policy planning administration. One gets an abbreviated but sound picture of the shifts in the management of an attitudes toward science and technology from the period of Russian domination in

the 1950s, to the Cultural Revolution of the 1960s, and through the severe changes of the 1970s. Of special note is the author's analysis of the roles of Mao and Chou in the initial shifts in policy after 1972.

There are excellent summaries of the discourse on the ideological identity of science (super- or sub-structure?) and scientists (of the worker's class?) and cognate questions such as the role of the party in science policy formation and implementation. Well outlined are perennial Chinese concerns with professionalization, where they affect the organization and practice of science. And we get a clear picture of the science orientations of the Cultural Revolution (populist, decentralized, and combining research with production) vis-à-vis the current one (elitist, centralized, planned, and specialized, with a laboratory focus and an emphasis on basic research). There is also a sensitive sketch of the generational tensions within the science community. Finally, convincing arguments are made for the considerable constraints the Chinese may have to overcome to achieve their ambitious goals: sufficient quantity and efficient use of manpower and funding, and the systemic integration of foreign technology.

This book provides informative and provocative reading for both university students and Washington officials. For the latter, the author concludes with some challenging and timely policy questions.

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ROSS TERRILL. *Mao: A Biography*. New York: Harper and Row. 1980. Pp. ix, 481. \$17.50.

Mao Tse-tung is one of this century's more over-studied dictators, and the only advantage of Ross Terrill's work over previous biographies is that it was completed after Mao died and can, therefore, tell the full story. The style is what is sometimes called journalistic, using short paragraphs, short words, elliptical sentences, and terms like "up front" and "society trip."

It is tempting to focus on style. In content the book is shot through with trivial errors of fact and interpretation, each one perhaps nothing in itself but cumulatively leaving the reader depressed. Because they are for the most part trivial, to list them at random would seem like nit-picking, while an attempt to correct them all might take another 434 pages. The style is a constant, irritating intrusion. It is as if Terrill, having decided to write a popular account, were taken with the fear someone would mistake him for a scholar. He is given, for no particular reason, to translating Chinese place names into

English, rarely felicitously and sometimes incorrectly. He refers to women of the early part of the twentieth century as Ms., although that title was not even invented until about 1970. He gratuitously uses offensive language, sometimes straining so hard for indecency that he becomes incoherent: "Jiangxi had been mere masturbation, alongside this full intercourse with the radiant bride of China (at least a portion of her) . . ." (p. 162). Just what kind of mental picture is this supposed to arouse? Yet when vulgarity is called for, as in literal translation, Terrill contents himself with officially sanctioned euphemisms (p. 313).

The notes contain references to extensive source material in Chinese and English, although Terrill rarely treats his sources in a critical fashion (unless the source contradicts the point he is trying to make). The text of the work uses the pinyin system of romanization (Mao Zedong, not Mao Tse-tung), but the notes promiscuously mix pinyin with the once standard Wade-Giles system, leaving an impression of unconcern.

The book does have its good points. Terrill's theme is based upon Mao's interpretation of himself as part tiger, part monkey. Terrill concentrates on the monkey, stressing Mao's personal life, as well as it can be reconstructed. This can be interesting, and the gossip-column slant compensates in part for the unsatisfactory political analysis and the apparent ignorance of culture and history. The interpretations of Mao's state of mind or tone of voice may not always ring true, but the evidence does not permit great rigor of inference about this kind of thing. Terrill has interviewed the poohbahs of the world and incorporates their impressions of Mao gained from their meetings with him. The description of the meeting of Mao and Gough Whitlam is hilarious, a small satirical masterpiece. Terrill's general judgments are sensible and balanced. His overall evaluation is that Mao "was a great leader but in many ways not an admirable character" (p. 430).

In the end, however, this is a trivial book. Terrill can write an intelligent biography of R. H. Tawney, and he can write about his own experiences in China in an attractive, literate way. Here it is as if he were bored by his subject and contemptuous of his readers. This is a poor job by someone capable of better.

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KEY-HIUK KIM. *The Last Phase of the East Asian World Order: Korea, Japan, and the Chinese Empire, 1860-1882*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xxv, 414. \$20.00.

This book presents, for the first time in English, a

detailed narrative of Chinese-Japanese-Korean relations during 1860–82. These dates mark the period in which the traditional pattern of the three countries' relations steadily eroded under the pressures of Western diplomacy.

Key-hiuk Kim, on whose dissertation the book is based, has done an impressive amount of research. His command, which few can equal, of Chinese, Japanese, and Korean alone would make the book a landmark. But he is not merely content with multiarchival research. He takes pains to correlate various sources and interpretations in order to present a well-balanced account. The result is a history that is free of emotionalism, chauvinistic biases, or dogmas. His use of certain nouns and adjectives does reveal where his sympathies lie; such terms as "talented . . . perceptive mind" (p. 290) and "patriotic men of intelligence and integrity" (p. 326) seem to be reserved for Koreans, whereas words like "treacherous and untrustworthy" (p. 20), "depravity" (p. 29), "predatory" (pp. 255, 336, and elsewhere) and "intransigence" (p. 338) describe other nationals. But with the exception of "predatory" (to be noted below), such expressions are not particularly jarring and do not detract from the smooth flow of the narrative.

A book of this nature should be evaluated not just in terms of the details it presents; of this there is no doubt. It is particularly informative on Japanese-Korean relations during the 1860s, and on Korean domestic politics throughout the entire period. An equally pertinent test would be whether the details add up to major reinterpretations. Here Kim would disappoint those looking for bold new generalizations. Neither his description of the traditional "East Asian world order" nor its breakdown departs substantially from standard accounts. He writes that the traditional Sinocentric order gave way to something new under the weight of the West's "predatory" expansionism; that the picture was complicated because the Japanese emulated the West by engaging in "predatory" activities of their own; that Chinese and Koreans were suspicious of both the West and Japan and sought to prevent their collusion; and that in the end China too deserted Korea as it sought to cope with the combined threat of the West and Japan by asserting its interests in the peninsula. These interpretations do not differ drastically from usual interpretations. The author should be commended, however, for portraying Korean officials, many of whom are little known to nonspecialists, not as tradition-bound bigots but as "rational actors"—primarily concerned with national security and defense and differing among themselves only with regard to tactics. It is because most Korean figures, as well as some Chinese and Japanese officials, are depicted as oriented toward their respective national interests

that one finds incongruous the author's quick dismissal of the West and Japan as "predatory." It would seem that one needs more than a coming together of rational Asians and predatory Westerners to explain the momentous events of the 1860s and 1870s.

The book is a welcome addition to the diplomatic history of East Asia. Readers who are looking for more than conventional diplomatic history can use the book as a point of departure, full of reliable data, to construct their own interpretations.

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JEFFREY P. MASS. *The Development of Kamakura Rule, 1180–1250: A History with Documents*. Stanford: Stanford University Press. 1979. Pp. xv, 312. \$18.50.

From the publication of Minoru Shinoda's *Founding of the Kamakura Shogunate, 1180–1185* in 1960 until the mid-1970s, there was virtually no serious scholarship on Kamakura Japan in this country. In 1974 Jeffrey P. Mass published *Warrior Government in Early Medieval Japan*, initiating a course of research that has raised the study of Kamakura Japan to a higher level. *The Development of Kamakura Rule, 1180–1250* is Mass's third volume on the period in six years. But there is more to come. In his introduction, Mass for the first time promises in writing what he has long said informally: *Kamakura Rule* is only volume three in a five-volume series covering the Kamakura *bakufu* from establishment to demise.

Those familiar with Mass's writings will easily recognize this volume. His greatest strength lies in his extensive use of land documents; no other Western scholar has approached his mastery of the sources for Kamakura history, and his work reflects that knowledge. *Warrior Government* dealt with the establishment of Kamakura *bakufu* institutions seen from the documentary evidence. *The Kamakura Bakufu* (1976) was precisely what its subtitle claimed: *A Study in Documents*—translated documents illustrating important developmental aspects of Kamakura warrior government. *Kamakura Rule* combines the approaches of the first two volumes.

Mass divides the book into three parts. Parts 1 and 2, in six chapters, form the narrative section. The first two chapters cover the Jōkyū War of 1221 and the settlement reached between Kamakura and Kyoto at the war's end. Chapters 3 through 6 deal with the evolution of Kamakura justice until 1250, a subject the author believes worthy of such extensive coverage because he feels, quite correctly, that judicial matters were the chief activity of the Kamakura *bakufu*.

Part 3 is derived from the narrative sections, consisting of translations of 144 of the most important

documents on which the first two sections were based. This is a valuable section for students of the period; the documents are important, the translations clear and accurate, the notes useful. Unfortunately, but understandably, the Japanese documents are not reproduced, so a student would have to spread the volumes of the Japanese *Kamakura ibun* out in front of him to learn something about the translation or decipherment of documents from this section.

Kamakura Rule is rich in detail previously unavailable in English, especially in the area of early Kamakura relations with the court at Kyoto, and Mass's discussion of Kamakura justice should remain the authoritative study for many years to come. A solid, heavily footnoted institutional study, *Kamakura Rule* reads unusually well. Mass has an engaging style, and by frequently posing and answering questions, he keeps the reader's attention—at least in the narrative section. Reading translated documents, on the other hand, requires great patience, strong motivation, and a long attention span, qualities that this reviewer unfortunately lacks. *Kamakura Rule* is an excellent study, recommended to all serious students of Japan; and Mass fans can take heart knowing that another volume is on its way.

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CONRAD TOTMAN. *The Collapse of the Tokugawa Bakufu, 1862–1868*. Honolulu: University Press of Hawaii. 1980. Pp. xxiv, 588. \$22.50.

The usual complaint about the Tokugawa shogunate (1603–1868) is that it exercised authoritarian rule over Japan. Historians of all stripes have scolded the regime for its proclivity to dictatorship. So strong was the shogunate (*bakufu* is the preferred term) that it became *primus inter pares*, propelling the Tokugawa above the other late feudal warlord families who had vied for the status of hegemon. The *bakufu* even controlled Japan's foreign relations and kept the country essentially closed for two centuries. Why, then, did the system suddenly collapse?

Conrad Totman's thorough study of this question complements his earlier book, *Politics in the Tokugawa Bakufu, 1600–1843* (1967), which showed how the system worked. The new volume deploys a massive formation of facts to reassert the crux of an interpretive model advanced some thirty years ago by Sakata Yoshio at Kyoto University and basically accepted by Western historians such as Albert M. Craig, Marius B. Jansen, and William G. Beasley. The argument turns on the abrupt overwhelming "imperialist intrusion" after 1850 (p. xiii). The Western powers with their demands for trade and

diplomacy overloaded the political process, disrupted local and regional economic patterns, and in the 1860s generated a fully fledged "systemic crisis" (pp. 479–81) that the *bakufu* could not disperse. It therefore collapsed.

The virtue of Totman's book lies in the sheer clarity with which he narrates this collapse and explains its phases. Rigorous and novel in periodizing, he waves off attempts to situate the causes of Tokugawa decline in the 1850s, then shows how the *bakufu*, no less than its rivals, concluded by 1866 that the old order was bankrupt. The major reform efforts of 1862 and 1866 actually hastened the Tokugawa collapse rather than retarded it, and the ruination of the *bakufu* disqualified its officials from articulating the new order that would need to be constructed. The old leaders thus not only fell but also lost the chance to shape the future, despite their superior knowledge of the outside world. It was their rivals who benefited by the sense of "ethnic danger" that impelled Japanese to react against the foreigners, and the Imperial Restoration of 1868 reflected the force of a vitalized "ethnicity"—a term the author prefers to nationalism (pp. 485–86).

Totman's book is so clear about assigning cause and effect relationships that sometimes it seems to resemble a set of propositions in formal logic more than an inductively based historical narrative. But this illusion is tribute to the author's organizing skill, and his abundant sources do support such a tightly woven causal fabric. A story can be told as many ways as music can be scored or a picture painted. Totman's story is neat, well paced, and heavily political.

Tocqueville wrote that the worst moment for an authoritarian regime comes not when its policies are working but when it puts its hand to reform. His observation fits Japan in the 1860s. Totman's notion of systemic crisis makes the *bakufu*'s demise appear more predetermined than it probably was, but the crisis was real and afflicted all facets of government operation. He has given us an authoritative account of the way a great political configuration can come apart when wholly new forces supervene.

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ROBERT R. SWARTOUT, JR. *Mandarins, Gunboats, and Power Politics: Owen Nickerson Denny and the International Rivalries in Korea*. (Asian Studies at Hawaii, number 25.) Honolulu: University Press of Hawaii. 1980. Pp. xvi, 192. Paper \$9.75.

This book presents a scholarly narrative of the diplomatic career of Owen Nickerson Denny, Vice President of the Korean Home Office and Director

of Foreign Affairs between 1886 and 1890. The most important primary sources for the study are the Denny Papers in Oregon and the relevant Korean- and English-language archives. In an interesting book based on his doctoral dissertation at Washington State University, Robert R. Swartout, Jr., analyzes Denny's activities in Korea, focusing particularly on his duels with the Chinese, but with due attention also to his dealings with the British, Russians, French, Japanese, and Americans. Swartout portrays Denny as an honorable man sincerely interested in helping Korea in the idealistic spirit of present-day Peace Corps volunteers. In this regard, Denny was more like George C. Foulk than Horace N. Allen. Despite some accomplishments, Denny was ultimately unable to achieve his two-fold aim—the independence and modernization of Korea—for a variety of reasons, many of which were beyond his control.

Swartout has made a significant contribution to our understanding of the international struggle of the 1880s for political and strategic power in this unfortunately situated peninsula kingdom. With regard to China's pretended seriousness about Korea's dependent status in the 1880s, Swartout agrees with the revisionist interpretation presented in my study of *Diplomatic Relations between the United States and Korea, 1866-1887* (1970) and in Key-Hiuk Kim's *Last Phase of the East Asian World Order* (1980). Swartout shows that China's interference in Korea in the 1880s was mainly for strategic reasons and had little to do with the suzerain-dependency tradition. Examining Melvin Frederick Nelson's criticism of Denny's thesis concerning Sino-Korean relations, Swartout correctly points out that Nelson offered "a one-dimensional interpretation" by accepting "Chinese pronouncements at face value without delving into the reasons behind those pronouncements," whereas Denny "overemphasized a legalistic approach" (pp. 113-14).

There are two important points that Swartout should have clarified. First, whether we interpret China's claim to suzerainty over Korea as "a pragmatic response" or not, it was largely China's inconsistent and incoherent policies toward Korea since the 1830s that caused "Korea's seemingly contradictory behavior." Second, Denny's role as "a cultural imperialist" in Korea should have been elaborated in terms of broader East-West relations by explaining in what way Denny's objectives were justifiable and desirable.

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HUGH TINKER. *The Ordeal of Love: C. F. Andrews and India*. New York: Oxford University Press. 1979. Pp. xxi, 334. \$17.95.

Few members of today's reading public have more than nodding acquaintance with the person whose life and work are the subject of this book. In India, however, the name of Charles Andrews is still revered. Indeed, the 1971 centenary of his birth brought forth a widespread tribute to him. Hugh Tinker sees in this appreciation the continuing recognition by his adopted country of Andrews's devotion to India and his selfless commitment to the welfare of its people.

Starting with Andrews's boyhood in Birmingham, the author traces his education through his student years at Cambridge, including the personal spiritual conversion that led to his ordination as an Anglican deacon. Andrews was drawn to India when, in 1903, he accepted a post on the staff of St. Stephen's College, a missionary institution in Delhi run by the SPG. He soon became aware of the racial discrimination that prevailed in India under the British Raj. Andrews felt that missionaries were obligated by Christian precepts to rectify the racial status quo wherever possible. When the SPG rejected his views as impractical, he broke with the church and began his life-long espousal of Indian rights. He also immersed himself in Buddhist philosophical doctrine, which he tended to equate with the teachings of Christ.

Early in Andrews's Indian experience he fell under the spell of Tagore and Gandhi, with both of whom he henceforth shared the hopes and frustrations of his own tormented soul. The Charles Andrews of Tinker's biography was, in truth, a singular personality. Generous to a fault, prone to impulsive action, he drove himself mercilessly in his efforts to rectify the troubles that afflicted India. When the going got rough, he invariably turned to Tagore and Gandhi, sometimes for advice, at other times for the aid and comfort of their spiritual kinship.

Yet it is evident that these two gifted men did not see eye to eye in their attitudes toward the political crises that beset British India during the 1920s and 1930s. To be sure, both Tagore and Gandhi as well as Andrews publicly condemned General Dyer's brutality in suppressing the disturbances at Amritsar. But Gandhi's decision to use noncooperation against the British evoked Tagore's disapproval. He believed that the mass appeal of Satyagraha would induce the terrorists to commit acts of violence and thus provoke further repressive measures by the government. Andrews, for his part, would have nothing to do with it; nor could he countenance Gandhi's inflammatory speeches.

In assessing Andrews's role in Indian politics, the author describes his contacts and relations with a succession of British viceroys. Among these was Lord Irwin. Andrews and Irwin established a viable relationship by virtue of their mutual respect for

each other and because they both favored a peaceful solution of the Indian political crisis. Andrews's correspondence apparently helped to allay Irwin's distrust of Gandhi, although Tinker shed no light on whether Andrews was instrumental in bringing about the Gandhi-Irwin Pact.

The concluding sections of the book show how Andrews's influence as a mediator declined with the resumption of the political deadlock that occurred after the failure of the Round Table Conference in London. Tinker shares Andrews's objections to the drafting of a constitution for India by the British Parliament. In Andrews's view, it should have been a creation of the Indian people. Whether such a consummation was feasible in the light of the bitter Hindu-Muslim conflict is a question neither Andrews nor his biographer chose to raise. Indeed, one of the book's few defects is the omission of the subject of Hindu-Muslim relations. By and large, Tinker deserves high praise for his masterly portrayal of India's unique British spokesman and friend. Regrettably, the book lacks two essential devices, namely, a glossary of Indian terminology and a map showing the major localities mentioned therein.

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JOHN G. BUTCHER. *The British in Malaya, 1880-1914: The Social History of a European Community in Colonial South-East Asia*. New York: Oxford University Press. 1979. Pp. xvi, 293. \$34.95.

"As happy a land as one could ever hope to find—a Tory Eden in which each man is contented with his station, and does not wish for change"; thus, in the words of one observer quoted (p. 22) in this fascinating and meticulously systematic study of its dominant European community, did many British see their Malayan colony. Whether things were really as idyllic (especially in intercommunal relations) as these enthusiasts sometimes liked to claim, and despite the hardships of tropical life of which they regularly complained (to win better terms of employment and home-leave provisions, from both the government and the rubber plantation owners), colonial Malaya was certainly a land in which it was good to be British.

Contrary to our own nostalgic era's temper, this is not an anecdotal but a disciplined analytical study. It treats many issues familiar to students of British colonial societies and history: racial prejudice, intercommunal antagonism, and segregation (the "color-bar") in employment, housing, and transport; the internal class and status stratification among the Europeans, including the relations between governmental and commercial groups; the role of sporting

and social clubs in cementing the European community's cohesion and in enforcing its values and standards; the position of European women and the relations of European men with Asian concubines, mistresses, and prostitutes; and the allegedly deleterious physical and also moral effects of the tropical climate, especially upon women and children, and the remedies sought through recourse to liberal home-leave provisions, the development of hill stations, and boarding-school education in England. What distinguishes John G. Butcher's account is the insightful way in which he relates all these issues to a single underlying conviction that explains them all, and much else too: the belief that British colonial rule was grounded, at least on a day-to-day basis, upon prestige, not naked power; that this required all British to display, at least outwardly, behavior that merited the respect of the ruled and underpinned that prestige; and that this in turn entailed the maintenance among all Europeans of a certain material standard of living as the indispensable foundation for proper behavior and the prestige or moral power it sustained.

Some discussion of homosexual as well as heterosexual relations between Europeans and Asians and some consideration of the prominent Scottish contribution to expatriate Malayan society would have been welcome. So, too, would attention to that whole structure of economic, social, and also psychological power that provides the theme and synecdochic title of the late J. G. Farrell's last novel, *The Singapore Grip*. But such pleas are perhaps capricious. To have subjected expatriate society, no less exotic than that of the locals, to an unsentimental and revealing dissection is achievement enough.

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GLENN ANTHONY MAY. *Social Engineering in the Philippines: The Aims, Execution, and Impact of American Colonial Policy, 1900-1913*. (Contributions in Comparative Colonial Studies, number 2.) Westport, Conn.: Greenwood Press. 1980. Pp. xxvii, 268. \$27.50.

Since 1960 there have been several important studies on American colonial policy in the Philippines. All except two of them dwell exclusively on the period between 1901 and 1913—the Taft or Republican years. To someone who has worked in the vineyard of Philippine history, it is understandable why that era is chosen by scholars. First, the Americans who went to the islands were voluminous writers. They were determined to preserve for posterity a detailed, well-documented account of their activities in the Philippines. Furthermore, the leading Fil-

ipinos of that time, especially Manuel L. Quezon, left behind a wealth of documentation recounting the workings of the American colonial system and their part within it. Second, the Republican years form a whole, which makes research easy. In saying this, there is no intent to detract from the recent monographs (familiar to Philippine scholars) that have contributed to our understanding of the relationship between the colonial administrators and the Filipino elite in the areas of Philippine independence, economic dependence upon the United States, and overall political maturity. A casual reader could be left with the impression that the United States, after a bloody war with the Filipinos, settled down to the task of regenerating them, and that the Philippine Islands were turned into a "showcase of democracy." Further, this burden was made easier by the generous assistance, albeit self-serving at times, of the Filipino elite. Thus, the viewpoint that the colonial administrators in Manila did something different from their European counterparts is developed. The American people were not interested in the exploitation of the islands or the people.

What Glenn Anthony May has done is challenge the belief that the United States was "an essentially successful colonial power" (p. xvii). If social engineering is the process of restructuring a society, then Americans failed to make an appreciable dent in Philippine society. After examining the principal American and Filipino personalities, May looks at the preparations for self-government, the education of the masses, and the development of an economy for the islands. In each of these areas, despite the appearance of progressive attitudes, Americans were not really interested in a "showcase of democracy." The United States created instead a docile and safe haven from which it could have a window on the rim of Asia.

Is May's viewpoint correct and that of other scholars off the mark? It is not that simple. Other scholars, myself included, have measured what Americans did in relation to the other imperial powers at that time. We were not blind to the inadequacies described by May. What this present study does is hold up for scrutiny the deficiencies of American policy in the Philippines. The reader is confronted with evidence that nothing was done to restructure Philippine society: the poor were not helped, landlordism grew, disparity in wealth increased, and economic dependence upon the United States was established. In sum, May presents scholars of the American "experiment" in the Philippines with a provocative framework for further research.

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UNITED STATES

MERLE CURTI. *Human Nature in American Thought: A History*. Madison: University of Wisconsin Press. 1980. Pp. xvii, 453. \$25.00.

Ever since Plato defined man as a featherless biped and Zeno produced a plucked chicken by way of refutation European thinkers have been trying to get a handle on that most elusive of all God's creatures. And in American thought, according to Merle Curti, the idea of human nature has been a major theme from the beginning and especially important in times of crisis. Definitions of human nature have not only satisfied idle curiosity; they have also provided guidelines for action. They shaped the communities founded by emigrants to the New World, animated the American Revolutionists, informed the work of the Constitution makers, inspired countless movers and shakers in every period, and have engaged the systematic attention of biologists, psychologists, anthropologists, and sociologists since the late nineteenth century.

Americans have toyed with just about every possible conception of humankind: as noble savage, as *homo economicus*, as fallen Adam, and as clockwork orange. They have also disagreed violently over the importance of instinct and habit, nature and nurture, matter and mind, and existence and essence in the life of the individual. Curti's *Human Nature in American Thought* is the first full-scale study of American notions about human nature from colonial times to the present. The canvas is enormous; the execution is painstaking. Only a historian with Curti's vast erudition could bring it off. He accompanies his learning with illuminating comments and fresh insights both in the text and in the footnotes on each page. He has made a major contribution to America's understanding of itself.

In an introductory essay, Curti carefully defines his terms. Then, instead of proceeding directly to the Puritans, he does something more interesting: he examines the views held by the earliest settlers and the effect of life in the New World on these views. Curti eventually gets around to the Puritans and also to the eighteenth-century American *philosophes*, nineteenth-century romanticists, pragmatists, and Social Darwinists, and to the twentieth-century behaviorists, Freudians, and sociobiologists. But throughout he pays close attention to the interaction between European ideas and American realities. He takes it for granted that the United States is an integral part of Western civilization but that it also possesses special qualities of its own. He seems, too, to share Charles Beard's view that "the world is largely ruled by ideas, true and false."

Curti's treatment of the opinions he deems most

significant in the history of American thought is eminently fair, balanced, and comprehensive. He appears himself to lean towards Dewey's hopeful view of human possibilities, but he refrains from denigrating contrary views. And he makes it clear that the last word has by no means been said on the subject. He has done so much here that it is probably unfair to ask for more. But there is one lacuna in his account: the treatment of human nature by American novelists, poets, and dramatists, especially in recent times. Unlike prose, poetry rarely makes the mistake of thinking of human beings as plucked chickens.

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CARL N. DEGLER. *At Odds: Women and the Family in America from the Revolution to the Present*. New York: Oxford University Press. 1980. Pp. xiv, 527. \$19.95.

This book is the most important comprehensive study in the field of social history of American women and the family to be published. The wide range of source material used, the incorporation of demographic material with the more traditional manuscript sources, and the broad chronological and geographical scope account for its comprehensiveness. The most impressive aspect of this study, however, is its sense of controlled knowledge of the whole period, of all the people, and of the processes surrounding this particular aspect of society. The concluding chapter of most scholarly monographs, where the historian grapples with the present and attempts to predict the future, is usually the weakest. But Carl N. Degler's command of his material makes his final statement on woman's "dilemma" one of the most convincing in the book. The thesis of the title is that women must always be, or at least have always been, at odds with themselves when their efforts for individual achievement conflict with their family roles. Starting with the Revolutionary period, when male and female roles and values became differentiated, various aspects of women's changing goals and achievements are studied, within the context of the family, or, more specifically, of women in the family rather than the family itself as the subtitle suggests.

Degler analyzes the woman as wife and mother, her attitudes towards marriage, sex, and motherhood, and the responsibilities she assumed even within a traditional family. He has some wonderful quotes in these chapters, where women express their views with such vehemence that "The Angel in the Home" should recede as a role model once and for all. Further chapters on abortion, women's organizations, the drive for suffrage, and women at work document changing roles and persuasively argue

that change was resisted and perceived as radical (and was, in fact, radical) when it seemed to threaten the family. Although most of his manuscript material is from middle-class white women, Degler includes chapters on the Afro-American and the immigrant woman, as well as data from farm women.

In making sense of this disparate material Degler occasionally relies on a kind of ideological germ theory ("With freedom and individualism in the air . . .") that is less than convincing. He avoids any literary sources, but often it is in literature that motivation, relationships, and attitudes are specifically described in ways that the manuscripts simply do not provide. He also seems to ignore the explanations given by the women he studies as not having any validity. There are gaps in the institutions studied, notably religious and voluntary groups. This is not surprising since there are almost infinite numbers of these institutions, each with attendant annual reports, periodicals, and correspondence, just as the number of diaries, family letters, and memoirs approaches infinity. Most historians recognize their finite nature and admit that they have only sampled the documents available. Degler gives the impression that he has read everything worth reading even when it is clear he has not. The thesis of the book places women firmly within the family, although some historians, admittedly mostly women, have expended considerable energy in proving that women moved in circles other than familial ones. There is a notable lack of anger in this book, unlike most on this topic. Perhaps this detachment is purely scholarly; sometimes it seems faintly Olympian.

BARBARA WELTER
Hunter College,
City University of New York

DAVID LEVERENZ. *The Language of Puritan Feeling: An Exploration in Literature, Psychology, and Social History*. New Brunswick, N.J.: Rutgers University Press. 1980. Pp. xi, 346. \$22.00.

The Language of Puritan Feeling offers an unusual and often illuminating perspective upon Puritanism in seventeenth-century New England by combining literary analysis of texts (mostly sermons together with advice books and some autobiographies) with post-Freudian psychological theory. To a lesser degree, David Leverenz tries to connect his interpretations with current views of social historians concerned with developments in the history of the family and of communities in New England. His primary focus is upon the first two generations of ministers, including Thomas Hooker, John Cotton,

Thomas Shepard, Increase Mather, and Samuel Willard, each of whom is examined in detail in two chapters devoted to fathers and sons. In addition, the penultimate chapter provides a view of Benjamin Franklin and Jonathan Edwards as eighteenth-century Puritans without, however, offering much new insight into their characters, roles, or contributions to the themes explored in earlier chapters.

The central theme of the book concerns Puritanism and masculinity: male roles, male authority, male styles, male fantasies. Leverenz's thesis is "that unconscious ambivalence about the father's authority, and a broader weakening of traditional norms for male identity, helped energize the Puritan vision" (p. 3). The initial chapter on why English Puritans hated stage plays focuses upon the preoccupation of men such as William Prynne with effeminacy and mixed sex roles, indicative of deep anxieties about male identity. Subsequent chapters explore various psychological themes related to masculine experiences, concerns, anxieties, and fantasies. His stress throughout is upon ambivalence and polarities. He argues that Puritan fathers, contrary to most assumptions, were seen by their sons as weak, so that the preoccupation with patriarchal authority was more a fantasy than a reality. Mothers, on the other hand, were recalled as loving and nurturing, thus setting up the dynamics of ambivalence characteristic of Puritan males. One of the results was the persistence of what Leverenz calls the "Family Romance"—"a rebirth into the higher family of God as good father and good mother combined" (p. 107). The other major result was the obsessiveness of their characters as well as their language. "Puritan sermons appear to be paradigms of the obsessive style" (p. 138) while both Willard and Mather reflect "two sides of the obsessive style" (p. 219). The analysis of Puritan obsessiveness constitutes one of the chief contributions of this study.

For many historians, David Leverenz's use of psychological theories and insights for the analysis of Puritan texts will be the most controversial aspect of the book. He is duly modest about the difficulties of using Freudian and post-Freudian analytical models, given the paucity of biographical evidence for virtually all the men he examines. But his insistence that "the idea of shared fantasy" (p. ix) permits such a psychological analysis is persuasive. The problem lies in his inferences from some of the texts cited: too often the evidence seems insufficient for the claims that he makes. Even readers sympathetic to the application of psychology to historical sources may be perplexed and dubious about some of the interpretations that Leverenz offers.

PHILIP GREVEN
Rutgers University

E. DIGBY BALTZELL. *Puritan Boston and Quaker Philadelphia: Two Protestant Ethics and the Spirit of Class Authority and Leadership*. New York: Free Press. 1979. Pp. xii, 585. \$19.95.

This book apparently was written part in anger, part in sorrow by one who resides in two departments at the University of Pennsylvania, sociology and history, and who is especially known for two earlier volumes, *Philadelphia Gentlemen* and *The Protestant Establishment*. In *Puritan Boston and Quaker Philadelphia*, which the author calls comparative history, E. Digby Baltzell offers a humane, well-written lamentation that Philadelphia and, by implication, the American Republic have rarely been so fortunate as to receive leadership from persons of high ability and motivation who naturally take the responsibility to guide their fellows. Baltzell uses only a few recent episodes in American affairs as reminder of how shatteringly important his contention is becoming for our democracy.

"I have tried to show," Baltzell says in summary (pp. 433-34), "that Bostonians, whether Puritans, Unitarians, Episcopalians, Catholics, Jews, agnostics, or atheists, have continued to be influenced by the hierarchical communalism of Puritanism just as their counterparts in Philadelphia are still infected by the egalitarian individualism of the Quaker founders of the holy experiment." This realization, he says, came as an "intuitive conclusion" (p. x) after thirty years of observation, and that for ten years he has been confirming it. Baltzell marshals evidence first into a discussion of class authority and leadership, acknowledging his debt to Alexis de Tocqueville and Max Weber. Thereafter, his analysis addresses such matters as European roots of American culture, the colonial experience, an age of transition, and, in a monumental section, institutional comparison during the national experience. Baltzell draws support mostly from biographical sketches and listings of individuals by status and attainment, all nicely showing how the best of Boston sought public office or societal authority outside of business, while that of Philadelphia did not.

What undergirds this often charming and always artless book is Baltzell's insistence that class is the essence of authority in any society and that it is the proper role of an upper class to exercise that leadership through the command of respect. He suggests this prevailed in Boston and not in Philadelphia because of contrasting ideological traditions.

Baltzell says that comparative history must be based on secondary authorities, which means that his impressions are drawn from someone else's works, thereby accounting for considerable error and misinterpretation among the numerous careers he discusses. Meanwhile, much labor goes toward applying a veneer of analysis by quantification, an

effort that ultimately seems as simplistic as the use of printed sources. Baltzell's success is his thoughtful and witty narrative, which moves relentlessly against the disgrace of Philadelphia. Unfortunately, this may not keep some readers from dropping the book in disgust because they will sniff elitism. That will be a pity. For while Baltzell may still be building upon his intuition, he nevertheless has been gripped by a concern no historian ought to wave aside—how *do* motivation and ability affect society? Although Baltzell has written a tract, it surely bears reading.

PAUL C. NAGEL
Virginia Historical Society

PAUL G. E. CLEMENS. *The Atlantic Economy and Colonial Maryland's Eastern Shore: From Tobacco to Grain*. Ithaca, N.Y.: Cornell University Press. 1980. Pp. 249. \$15.00.

This ambitious monograph transcends the usual limits of such studies. The subtitle, *From Tobacco to Grain*, accurately identifies the heart of the work, while the main title suggests the reason for its broader scope.

Part 1, "From Boom to Stagnation, 1620s to 1713," sketches the Eastern Shore setting from first settlement, when tobacco prices still stood high enough to attract planters great and small, down through the years of stagnation when profits from planting dropped to levels that barely supported producers. Paul G. E. Clemens brings under scrutiny immigration, population growth, the land system, market organization, and economic opportunity—all closely touched by the stagnation that plagued these counties through the first three inter-colonial wars.

Part 2, "Revitalization, 1713–1776," accounts for the final sixty years of colonial dependency, when the economy revived under the benign influence of rising tobacco prices and the introduction of wheat as an alternate cash crop. This transition to wheat production on the upper Eastern Shore, though often mentioned by historians of the Chesapeake, has never before come under the kind of analysis Clemens devotes to it. His quantitative treatment discloses the limits as well as the parameters of wheat production. At his hands what might have been simply agricultural history becomes full-bodied economic history with due attention to markets, profits, the interplay of rural and commercial sectors, and—very importantly—the social order and the vital organizing element, the merchant-planters.

This monograph, then, is geographically a micro-study, with all the advantages of that genre, over a time period sufficiently long to manifest secular trends often lacking in similar works. The author by

no means neglects short term fluctuations, of pressing concern to colonial producers, but even a glance at the tables and graphs richly strewn through the text suggests the importance he attaches to the longer run. Anyone who has worked in seventeenth- and eighteenth-century economic history will admire Clemens's skill in extracting the last shred of usable evidence from these frequently intractable sources for his tables. Moreover, his micro-analytical techniques enable him to avoid the pitfall of oversimplification.

Well-conceived and informative, this study of the complexities of a hitherto imperfectly known subject has a minimum number of blemishes. A few tables (notably Table 14, p. 145) are not well devised; at one point (pp. 179–81) the graphs and text are not fully congruent; and occasional careless sentences distract the reader's attention from the argument. For conception and execution, this monograph stands in the front rank of the published research on Chesapeake history that flowered at the Maryland Hall of Records in the last years of Dr. Morris L. Radoff, its fostering custodian. He would, I am sure, lead the applause for Clemens's work.

AUBREY C. LAND
University of Georgia

GARY B. NASH. *The Urban Crucible: Social Change, Political Consciousness, and the Origins of the American Revolution*. Cambridge: Harvard University Press. 1979. Pp. xv, 548. \$18.50.

The Urban Crucible is an essentially two-dimensional analysis of the comparative development of Philadelphia, New York, and Boston between 1690 and 1776. One dimension considers the "urban social process" (p. xi) and consists of six chapters comprising roughly 40 percent of the volume. Gary B. Nash's conception of social process is rather narrow: it does not include such topics as social geography, social infrastructures (as manifest in institutions, organizations, and social networks), or, except in a cursory way, material conditions and styles of life. Rather, he focuses very largely upon a single aspect of social development: the changing distribution of wealth and its interrelationship with shifting economic and demographic conditions. Although his data is much fuller for Philadelphia and Boston than for New York, his analysis of this subject makes a highly significant contribution to the reconstruction of early American social history and demands the serious attention of all scholars in the field.

At the most general level, the author records an erratic but considerable transformation in the social dynamics of all three towns. In 1690, he argues, they were vertically organized and highly deferen-

tial. Elites were still in a formative stage; poverty was rare and confined mostly to widows, orphans, and the disabled; and extremes in wealth were not very great. Communal solidarity was high, and opportunity and economic expectations modest. Over the next 85 years, opportunity gradually decreased for those at the middle and lower levels of society, wealth became far more heavily concentrated in the hands of a conspicuous elite, and poverty increased to the point that the "old familial system of relieving want . . . gave way to [highly impersonal] non-familial institutions" (p. 329). Among other results, according to Nash, communal solidarity and habits of deference gradually weakened, competition supplanted consensus, and social consciousness came to be based more "on horizontal rather than vertical divisions in society" (p. xi). Nash is careful to point out that this transformation did not occur according to some "even-paced or linear formula" (p. xi). As he shows in rich detail, there were significant differences in timing and degree from town to town and from one period to another according to a number of variables, the most important of which seem to have been hinterland productive capacity, degree and nature of war involvement, and character of relationships to external markets. The author's generally persuasive account of the differential operation of these and other variables is too complex to be briefly summarized. But Boston's socioeconomic situation deteriorated much earlier and much further than did those of Philadelphia and New York, neither of which suffered substantial or sustained socioeconomic dislocations until the late 1750s.

Nash provides a second dimension through a detailed recounting of the political history of the three towns. Alternating with the chapters on social change, seven remaining chapters explore the interconnections between those changes and politics between 1690 and 1776. Concentrating upon the analyses of those "controversies around which urban political innovation and radicalization occurred" (p. 129), Nash argues that "four generations of political development in the American commercial centers" prior to 1765 "set the stage for a ten-year internal struggle for political control among the various social elements protesting English rule" (p. 292), a struggle marked by "the rise of a radical political consciousness . . . the crumbling of the elite's cultural hegemony, and the final assaults on the old political order" (p. 338). Especially for the last fifteen years of the colonial period, Nash has uncovered significant displays of lower-class consciousness. More important, he has linked them successfully with lower-class political behavior and thereby significantly enriched our understanding of the complexity of the American Revolution. In view of the fact that the urban gentry lost control

only in Philadelphia, however, his characterization of pre-revolutionary political struggles within the cities as a "profound social upheaval" (p. 340) will strike many readers as hyperbolic.

By building his study upon the premise that in fact as well as in theory habits of obedience and communal solidarity characterized social relations in the towns in 1690 and earlier, Nash ignores the implications of most recent scholarship and much of his own evidence that authority was fragile and consensus tenuous throughout the seventeenth and eighteenth centuries in most colonial situations, even in Puritan New England. He thus regrettably perpetuates the antique and serious distorting myth, itself largely a Puritan artifact, of an early harmonious golden age from which there was a subsequent long-term declension. An obviously technical work that will be of interest mainly to scholars and students, the volume was unfortunately designed without consideration for its audience. By placing the tables, which are integral to the argument of the book, and the notes, almost all of which contain substantive elaborations or modifications of the text, at the end, the publisher made the volume exasperatingly difficult to use.

JACK P. GREENE
Johns Hopkins University

JOSEPH J. ELLIS. *After the Revolution: Profiles of Early American Culture*. New York: W. W. Norton. 1979. Pp. xvi, 256. \$16.95.

During the last decade or so, a number of scholars have sought to explain why the Revolutionary and Early National periods, so rich in political and constitutional accomplishment, produced little of lasting value in the creative arts. Offered as explanations have been the preoccupation of creative energies with the pressing tasks of nation-building, the ideological bias of revolutionary republicanism against "luxurious" and "decadent" forms of cultural expression, the absence of private and public patronage of the arts, and the tenuousness of artistic communities capable of providing essential psychological and critical support. In this interesting and thoughtful volume, Joseph J. Ellis offers his own perceptive thoughts on the matter.

Members of the revolutionary generation, Ellis argues, expected that America would become the cultural as well as the political capital of the world. People believed that the new nation, energized by the "vitalizing power of individual freedom," was "on the verge of a cultural revolution" (p. x). Yet by 1830 it had become clear that nothing of the sort had happened. Mrs. Trollope's acidulous comments

on the banality of American culture, high as well as low, made that clear.

What had gone wrong? The problem, Ellis explains, arose from a long train of "social and attitudinal changes" (p. xi) promoted by the revolutionary experience and furthered by the nation's accelerating economic and demographic growth. America's new "liberal creed" (p. xii)—with its emphasis on individual autonomy and self-expression, equality of opportunity, social mobility and change, popular sovereignty, acquisitive capitalism, and an assumed connection between talent and the possession of material wealth—proved a hostile rather than a supportive environment for the development of high culture.

As presented in the book's first two chapters, Ellis' argument is provocative, but remains too general to persuade fully. It comes alive, however, in the four "profile" chapters that follow, where Ellis illustrates how these social and cultural changes deadened the creative sensibilities of four would-be artists—Charles Willson Peale, Hugh Henry Brackenridge, William Dunlap, and Noah Webster—by absorbing their energies in the celebration of national development and the playing to mass tastes. In part, Ellis sees the four as victims of their times. Yet he detects ironies at work as well, for by assuming with others of their generation that artistic, economic, and political development were interrelated and compatible rather than autonomous processes, and by denying the "inherent antagonism" (p. 216) between artistic values on the one hand and popular opinion and the ethos of the marketplace on the other, they unknowingly committed a form of "cultural suicide." Not until Emerson, he concludes, would American artists and intellectuals develop the conscious separation from society prerequisite to developing a significant high culture.

There are powerful, and not fully explored, assumptions here about the connections between art and alienation. And it is not altogether clear why Emerson (and presumably Melville and others) found room to exercise their artistic talents while their predecessors did not. But Ellis understands the connections between social and cultural context and the creative life, and he helps us understand what those connections were during the years of the early republic.

JOHN HOWE
*University of Minnesota,
Twin Cities*

CHARLES F. NOLAN, JR. *Aaron Burr and the American Literary Imagination*. (Contributions in American Studies, number 45.) Westport, Conn.: Greenwood Press. 1980. Pp. xiv, 210. \$22.50.

As a field of inquiry that attempts to transcend the limitations of traditional disciplines, American Studies can never afford to tie itself to a mechanical or formulaic methodology. The best studies of American culture—by Henry Nash Smith, David Potter, and Sacvan Bercovitch—use diverse, complex methodologies and are so imaginative in conception that they fail to provide ready models for subsequent scholars. Charles F. Nolan, Jr., and Greenwood Press should have noted these facts before they produced *Aaron Burr and the American Literary Imagination*.

Nolan's book is a pedestrian summary of the popular images of Aaron Burr that predominate in 82 plays, stories, and novels published since Burr's death in 1836. Chapter 1 covers the facts surrounding the basic controversies in Burr's life; chapter 2, the contemporary treatment of Burr in the press; chapters 3 and 4, his characterization in drama and fiction; and chapter 5, the relationship Nolan claims to have found between popular images of Burr and "the darker side of American life" (p. xiii).

Not only is this organization for the book conventional in the extreme, it is executed poorly as well. Though chapter 1 purports to give the key facts of Burr's life, Nolan brings in others by the back door in later chapters when they suit his defense of the "real" Burr. Moreover, his case for a distortion of Burr-reality might actually have been stronger if he had kept the facts congested in chapter 1, since the literary images of Burr that appear in chapters 2 through 4 are not really distortions of the facts given in the first chapter. Eventually, after constantly postponing the reader's expectations, Nolan presents in chapter 5 (but even there only briefly) his central thesis that Burr has become an "American symbol" in whom "some of American society's deepest fears are dramatically revealed" (p. 157).

Those "fears," however, turn out to be either a conscious, understandable aversion to traitors and libertines or a sensible, revisionist awareness of political victims. Meanwhile, Nolan never asks why major writers (as opposed to his gallery of minor ones) have all but ignored the figure of Burr; how much those minor writers were influenced by standard historical accounts of Burr rather than their own "nightmares"; or why the writers who did deal with Burr were so inept, and if Burr's life might in some way invite inept treatment. Instead of revealing the "gigantic figure" of Aaron Burr "haunting" American literature (p. 182), Nolan gives us a Burr who is a ready-made stock figure of Western middle-class romance.

DAVID STINEBACK
University of Rhode Island

ELIZABETH P. MCCAUGHEY. *From Loyalist to Founding Father: The Political Odyssey of William Samuel Johnson*.

New York: Columbia University Press. 1980. Pp. xi, 362. \$22.50.

William Samuel Johnson's "political odyssey," as the subtitle calls this study, is pretty much that. Although his relations with his father, Samuel Johnson, the prominent Anglican spokesman, are set forth fully, we learn almost nothing about William Samuel's children or his relations with them. He was a leading lawyer, whose reputation extended beyond Connecticut, but little is said about his preparation for the bar or his law practice. One cannot fault the author for staying within chosen restrictions, perhaps, but Johnson's role in Connecticut's and the nation's politics was in no sense central. He was a member of the Stamp Act Congress, served in Connecticut's upper house (the first Anglican to do so), did duty as the colony's co-agent with Richard Jackson in London (1767-71), and sat in the Confederation Congress, the Federal Convention, and his state's ratifying convention. He also served briefly in the United States Senate and wound up his career as president of Columbia University.

This list of services is impressive, and not all have been mentioned here; but despite the voluminous Johnson papers, the author is unable to document much in the way of decisive action or leadership. Johnson was a prudent, cautious man, determined to be successful in politics and therefore not much given to taking chances or firm public positions. The retelling of revolutionary events offers nothing really fresh; Johnson seems cast mainly in the role of commentator, one who is neither original nor profound. We learn of his disappointment with the resolutions of the Stamp Act Congress, his distaste for John Wilkes, his relations with the Susquehanna Company, his disgust with the abandonment of nonimportation when all but the tax on tea was repealed, and his support for the so-called Connecticut Compromise in the Federal Convention. Much of the time he is left on the sidelines as the author fills out her narrative. The chapter on his presidency of Columbia University is almost entirely an account of what others did to reshape that institution. The establishment of an American episcopate was likewise the work of others.

Johnson's loyalism was of the quiet sort—a refusal to go along with independence that led him to resign public offices and the practice of law. He was not without principle, but he sought sedulously to avoid controversy. Briefly he was put under arrest in 1779 when the British were raiding Connecticut towns. Since he was not a man to say or do anything dramatic, the reader's interest flags, and he wonders whether this life is worth the retelling.

The notes reveal a good deal of work in manuscripts and printed sources and but few gaps in the

secondary literature cited. The author has supplied an excellent index. Her industry is not enough, however, to overcome a dull subject.

ROBERT J. TAYLOR
Adams Papers,
Massachusetts Historical Society

JOHN E. O'CONNOR. *William Paterson: Lawyer and Statesman, 1745-1806*. New Brunswick, N.J.: Rutgers University Press. 1979. Pp. xv, 351. \$23.50.

This successful study of New Jersey's leading figure in the early Republic lifts William Paterson from relative obscurity to a secure place among the nation's founders. If not among the men of first rank, Paterson clearly deserves a place in the same tier with such talented and successful men as C. C. Pinckney, Rufus King, R. R. Livingston, Monroe, and Elbridge Gerry who have also been subjects of recent biographical studies. Students of the Confederation can easily identify Paterson as author of the New Jersey or small state plan in the Constitutional Convention, though it is a telling commentary on what we have thought of Paterson's significance that he does not rate even an index entry in Jackson Turner Main's *Antifederalists*; many would recall that he was appointed to the Supreme Court by Washington; but that he was also active in his state during the Revolution as attorney general, one of New Jersey's first United States senators, governor and codifier of the laws of New Jersey, a national jurist of importance, and presiding judge in the trial of Matthew Lyon would escape most. It is tempting to elevate Paterson's importance by recalling that he was also offered the State Department by Washington until one remembers that Pickering was ultimately chosen.

Paterson's curriculum vitae suggests distinction, accomplishment, and talents, yet he is a poor subject for a biography. He lacked style, audacity, intellectual keenness, and humor—he played life close to the vest. Alas, there is no interesting twist to his movements from conservative lawyer to revolutionary, from champion of small state interest to Hamiltonian nationalist, or from defender of the Sedition Act to accommodating moderate in the early days of Jefferson's presidency. Paterson moved with the times, and even a biographer with Dangerfield's literary talent would be sore pressed to enliven his story. This is good, sound history, however, and there is much to be gained in reading it.

The author places Paterson squarely within the context of New Jersey history, whose dimensions are now sharp thanks to much excellent work completed in recognition of the recent Bicentennial. We learn little about Paterson's legal training but enough about his legal experience and his educa-

tion at Princeton to understand his strong sense of contract, his exasperation with developments that ran contrary to it during the 1780s, and the otherwise baffling combination of state particularism and nationalism that he displayed in the Constitutional Convention, his "finest hour" in the author's judgment.

The so-called "New Jersey Plan" submitted by Paterson to the Convention was not his ideal frame of government but a ploy to force the large state champions of the Virginia Plan to compromise sufficiently to allow the small, land-poor states to retain a viable share of sovereignty. Once the "great compromise" was accepted by the large state majority there was no impediment to Paterson's development as a nationalist, a Hamiltonian, and a Federalist. He was one of the leading figures in the ill-fated manufacturing establishment at the Great Falls of the Passaic and throughout his life a devoted champion of his state's economic prosperity.

Brevity forbids doing full justice to either the book under review or its subject's contributions to the building of the early Republic. The strongest chapters are those dealing with the Federal Convention, chapter 7, with the reform and codification of New Jersey law carried out by Paterson while on the Supreme Court, chapter 10, and with his service on the bench, chapter 12. The author argues convincingly that William Paterson conducted himself with discretion, wisdom, and political acumen as an associate justice; that he clearly foresaw the need for judicial review of national legislation in anticipation of *Marbury*; and that the strategy behind his decision in *Van Horne's Lessee v. Dorrance* (1795) was essentially that of Marshall in the more famous case.

The bibliographical notes display erudition and a greater sense of critical judgment than O'Connor allows himself in the text itself, and the attempt to relate Paterson's actions and decisions to the most well known of recent theses interpreting the early national period is somewhat strained.

STEPHEN G. KURTZ

Phillips Exeter Academy

WALTER T. DURHAM. *James Winchester: Tennessee Pioneer*. Gallatin, Tenn.: Sumner County Library Board. 1979. Pp. xii, 281. \$12.95.

Born in 1752, James Winchester grew to manhood in his native Maryland. He spent seven years as a soldier in the American Revolution, three as a prisoner of war. At the time of his discharge he had attained the rank of captain. In 1785 he moved to Davidson County, North Carolina, in the heart of the region that later became Tennessee. He held various public offices in Sumner County, which was

separated from Davidson in 1786, and commanded its militia as a lieutenant colonel.

In the North Carolina Convention of 1788 he was one of only two westerners who voted in favor of the ratification of the Federal Constitution. Unlike some of his contemporaries, Winchester never seriously considered a separation of the western country from the United States. He recognized the need for a strong central government and put his trust in it.

By appointment of President Washington, Winchester served as a member of the legislative council of the territory of the United States southwest of the Ohio River at the same time rising to the rank of brigadier general in its militia. When Tennessee became a state he was elected to its senate. The first action of that body was the choosing of James Winchester as its presiding officer.

In 1812 he was commissioned as a brigadier general in the regular army. With some justification, the author blames his surrender at the Raisin River upon the failure of General William Henry Harrison to supply and reinforce him. The massacre of some of the prisoners resulted from the very loose control that the British commander, Henry Proctor, had over his Indian allies.

Imprisonment in Canada followed this disaster. After his release he was assigned to the Southern Department where he served under his Tennessee friend, General Andrew Jackson. His failure to save Fort Bowyer at Mobile from capitulation was not criticized so bitterly by Jackson as his surrender in the North had been by Harrison.

Winchester spent the remainder of his life as a Tennessee planter, surveyor, merchant, industrialist, and steamboat operator. With his son Marcus representing him at the site, he played a major role in the establishment of Memphis. He died at Cragfont, his Summer County mansion, in 1826.

This book is a worthwhile contribution to the history of the American frontier and is based upon a good selection of both primary and secondary sources. Pictures of Winchester and members of his family add interest, and the maps are helpful. The index seems to be adequate.

CHARLES G. TALBERT

University of Kentucky

JOAN WELLS COWARD. *Kentucky in the New Republic: The Process of Constitution Making*. Lexington: University Press of Kentucky. 1979. Pp. 220. \$16.50.

Kentuckians take a perverse pride in asserting that their politics are the damndest. Joan Wells Coward's detailed account of the drafting of the first two state constitutions provides a new measure of the accuracy of that belief. The first (1792) Kentucky

constitution marked the culmination of a decade of disputes within and without the commonwealth and was adopted at the tenth convention held during those years. The founders included an unusual provision calling for a second constitutional convention in seven years, if voters at two consecutive elections desired one. This invitation was not intended to encourage extensive changes in the basic framework of government but to provide an opportunity to establish new constitutional offices that might be filled by newcomers to the frontier. The first constitution therefore had little chance for permanence, and the clause added a major item to the agenda of political debates.

It was a full agenda without the question of another convention, because Kentuckians disagreed with each other on several fundamental issues: whether slavery should continue, whether manumission should be made more difficult, and, most important, whether the barons of the Bluegrass were obligated to accept those they considered their social inferiors—particularly settlers in the western and southern parts of the state—as their political equals. (Kentuckians also disagreed with the national government, but Coward does not tie in those issues to local politics.)

Most students of Kentucky's early history have agreed that land was what Kentucky was all about, but it seems that the opportunity to build a political career was as compelling a motive for immigration as the desire to acquire property. The frequent clashes of personal ambitions perhaps account for the apparent inconsistencies in the public positions its citizens advocated. To find patterns to explain political behavior in this complicated society, Coward imaginatively uses tax lists and legislative journals (as well as standard primary sources) to construct tables illustrating landholding, slave ownership, representation, and—for the 1799 convention—voting charts and factor analyses.

It is an irony of Kentucky historiography that its most popular history was written by Humphrey Marshall, an arch-Federalist harpooner among Jeffersonian whales. Although acknowledging Marshall's partisanship, Coward relies heavily upon his interpretations. One might also question the alleged unavailability of early census data, whether the frontier was as disorganized as it is portrayed, and the isolation of local from national events. In any case, Coward brings new evidence that at least during the 1790s Kentucky politics earned the dubious distinction frequently claimed.

MARY K. BONSTEEL TACHAU
University of Louisville

LAWRENCE A. CREMIN. *American Education: The National Experience, 1783-1876*. New York: Harper and Row. 1980. Pp. xii, 607. \$29.95.

In the years between 1783 and 1876, according to the second volume of Lawrence A. Cremin's projected three-volume history of American education, "there emerged . . . an authentic vernacular in education," one of America's "two or three most significant contributions . . . to the advancement of world civilization" (p. xxiii). In these years "the signal achievement of popular education . . . was to help define an American *paideia* in education and teach it to a polyglot nation across a continent" (p. 497).

The "distinguishing features of American education" were "popularization and multitudinousness" (p. 471), meaning the diffusion of a variety of formal and informal educational agencies throughout the population. As a result of its distinctive features, American education was widely accessible by international standards and Americans among the most literate people in the world. Their literacy, according to Cremin, was "liberating." "Undoubtedly," he asserts, "the trend toward liberating literacy was the most fundamental outcome of the American system of popular education during the first century of national life" (p. 482).

A short review cannot do justice either to the book's contributions (primarily its attention to non-school aspects of education such as newspapers, voluntary associations, and churches) or to its limitations. However, it is important to recognize some of the latter.

First are inconsistencies. Cremin argues that American education had identifiable results and a distinctive ethos. Yet he contends that there "was a kind of formlessness about American education" (p. xxv). He often refers to the variety characteristic of nineteenth-century America but writes about "Americans" and the "American family." He claims to "have tried steadfastly to avoid the related sins of Whiggishness and anachronism" but presents his story as the emergence of a liberal and liberating tradition. He defines education as "deliberate" and "systematic" (p. ix) but in the course of his argument makes education all of life, truly ubiquitous, portraying, for instance, the Civil War as a massive source of popular education.

Cremin concentrates most on the history of ideas and attempts early in the book to define a republican tradition within American education (not, it should be said, republican in the sense now being used by historians of artisans in the early nineteenth century). To accomplish his goal Cremin assimilates Horace Mann to Thomas Jefferson, ignoring the conflicting aspects of their ideas and the major shift in the social theory of education that had occurred by the middle of the nineteenth century. He thereby creates a tradition that never existed and misses an essential transformation in the case for popular education.

In order to buttress his argument, Cremin dwells at length on the essays on education submitted to

the American Philosophical Society in 1795, even though they had no influence whatsoever, and subordinates to one paragraph late in the book the major innovation of the period, the Lancasterian (or monitorial) system that was welcomed and diffused throughout British and American cities in the late eighteenth and early nineteenth centuries as a way of providing cheap instruction for the poor in a decidedly nonrepublican spirit.

Although one chapter in the middle of the book deals with American economic development, presenting the major trends as themselves educational forces, Cremin does not set his history within any systematic exposition of American social and economic development, and he does not attempt to discern patterns within the variety that is his theme. Serious attention to the social history cited in the long bibliographical notes could have helped him connect educational "configurations" (to use his term) with particular strands in social and economic development. As it stands, his book reads as though the work of a generation of social historians on his period barely exists.

For the most part Cremin celebrates the history of education. Only with respect to blacks and Indians does he seriously criticize American behavior and attitudes. Even here, however, the treatment at times is curious. For instance, education during Reconstruction receives only four pages. These stress the black desire for education and Northern attempts to spread the national *paideia* but omit the acquiescence of Northern educators in the establishment of segregated school systems and their collaboration with Southern conservatives.

From reading Cremin's book, one would have little hint of the violence, poverty, corruption, and racism that marked late nineteenth- and early twentieth-century America, let alone an earlier period. For he concludes that by 1876, "the nation had been tested in the crucible of civil war and had endured. The dream of the founding fathers was now capable of realization, on a sound national basis; and, as the founding fathers had themselves understood, a new order of education would be at the heart of the achievement" (pp. 510-11). Thus are Whiggism and anachronism avoided.

MICHAEL B. KATZ
University of Pennsylvania

CRAIG L. SYMONDS. *Navalists and Antinavalists: The Naval Policy Debate in the United States, 1785-1827*. Newark: University of Delaware Press. 1980. Pp. 252. \$17.50.

As the last forty years have amply shown, military planning is no simple task, complicated as it is by questions of cost, lead time, policy differences, and

the shifting hazards of an unpredictable world. These same problems confronted the government during the forty-odd years from the Algerine crisis to the 1827 act for the improvement of the navy. Dependent on its foreign trade and merchant marine, the young nation found its citizens held for ransom by Muslim potentates, its Mississippi exports threatened successively by three great powers, its Atlantic trade the prey of the French and British, and its Caribbean commerce annoyed by pirates.

In the search for a naval policy appropriate to these changing circumstances, some comfort was early derived from the country's fortunate position outside of Europe. But the putative advantage was largely nullified by the overwhelming presence of Europe in America and by economic symbiosis. Political approaches to a solution were impeded by regional attitudes and partisanship in the Anglo-French struggles. Force design was complicated by the very different requirements for coping with piratical states and great maritime powers. In the upshot the extreme solutions of gunboats and ships of the line were rejected in favor of a commerce-protecting (and commerce-raiding) cruising navy of frigates, sloops, and schooners.

In tracing these developments, Craig L. Symonds has worked carefully through the congressional debates. As framework within which to deploy the arguments, he relies on the dichotomy of his title. The navalists, largely Federalist, urged peacetime preparedness and a naval establishment that would contribute to "national glory" (p. 205), deter aggression, protect commerce, and influence the European powers. The antinavalists wished to "protect America's anonymity" (p. 13), opposed a systematic defense policy, and variously opposed and supported naval action in defense of commerce and national rights (pp. 25, 123, 177, 231). But these categories are not overly helpful, masking as they do the shifts and developments in the arguments as old difficulties were solved or evaded, new ones arose, and experience came to modify theory.

Specialists will be interested in the details of congressional debate, especially in connection with the revenue bill of 1816. Those less familiar with the subject may find the chronology elusive and the background of debates and debaters obscure. The desire of some for a navy that could serve as "arbiter of Europe in America" seems at times inflated into a wish to participate in European broils. The inattention to naval developments on the Lakes is surprising. The style is generally straightforward and clear. The proofreading, not up to what one expects of a university press, offers such novelties as a U.S. sloop of war named the *Eire* (pp. 186, 252).

JAMES A. FIELD, JR.
Swarthmore College

JOHN K. ALEXANDER. *Render Them Submissive: Responses to Poverty in Philadelphia, 1760-1800*. Amherst: University of Massachusetts Press. 1980. Pp. xi, 234. \$14.00.

This is a book with an unambiguous thesis—that in Philadelphia during the revolutionary period the elite worked to keep the laboring poor in check by using poor relief, medical assistance, and education as instruments of discipline and moral management. This quest for control was urgent in upper-class minds because of the revolutionary rise of class antagonisms and the erosion of lower-class deference. Appalled at the widening of political rights under the radical Pennsylvania constitution of 1776 and at the social eruptions of the era that raised the specter of mob government, the elite embarked upon a crusade “to render them submissive” and thus to “defuse what they perceived as the potential danger from below” (p. 47).

John K. Alexander bases his investigation of upper-class attitudes toward the poor on meticulous newspaper research, for it was in the public prints that he found the richest discussions of poverty and its relief in the latter part of the eighteenth century. The other body of material that he has thoroughly combed consists of the records of public and private relief agencies and organizations formed to provide medical assistance, legal aid, and education for the laboring poor.

The argument that controlling the poor, while relieving their miseries, was motivated by the elite's perception of the political danger they represented is somewhat overdrawn. It was generally not the poorest of Philadelphians who were politically mobilized after the revolution but laboring people above them in the social structure. Moral control, however, was very much in the minds of the upper class, and it was accompanied, as Alexander shows, by a reformist impulse that was also a part of the revolutionary process. While some Philadelphians saw the poor as vicious and irredeemable, others who were attuned to environmentalist theories believed their condition was rooted in lack of opportunity and dismal prior circumstances. Thus, reformers worked to alleviate the miseries of Philadelphians at the bottom by improving jail conditions, curbing the retailing of justice, and revising Pennsylvania's penal code so as greatly to restrict corporal and capital punishment, lessen imprisonment for small debts, and end incarceration of persons acquitted of criminal charges who could not pay court costs. Similarly, reformers founded the Philadelphia Dispensary for outpatient treatment of those too poor to afford doctors, assisted manumitted slaves through the Pennsylvania Abolition Society, established charity schools for chil-

dren of laboring parents, and launched a movement for free public education. All of these strands of reformist activity are thoroughly examined.

Alexander's book adds to the growing literature on poverty in pre-industrial America and also makes an important contribution to the social history of revolutionary America. He illuminates the resiliency and determination of the postrevolutionary upper class to reassert itself in the new nation's largest city and gives the most detailed analysis yet available on how urban Americans responded to widespread poverty in the first decades of its appearance.

GARY B. NASH
University of California,
Los Angeles

LOUIS C. HUNTER. *A History of Industrial Power in the United States, 1780-1930*. Volume 1, *Waterpower in the Century of the Steam Engine*. Charlottesville: University Press of Virginia, for the Eleutherian Mills-Hagley Foundation. 1979. Pp. xxiv, 606. \$24.95.

Historians who ignore Louis C. Hunter's history of industrial power in the United States will have a far less compelling concept of the sweep of American history. Too often, surveys of this history have moved from agrarian crafts and early steam transportation directly to urbanized industry rooted in steel and steam. In so doing, they pass over intervening decades when the scene of industrialization was at widely scattered waterpower sites.

Hunter has recovered this historical landscape for us, historical country long obscured by the smoking steam engine and distorted by romanticized impressions of mill wheel and stream. The era of waterpower, which lasted until the 1860s, involved empirical inventors, systematizing managers, and optimizing engineers. They are not to be ignored or sentimentalized. There were numerous small mills grinding, fulling, stamping, and rolling in the countryside from the time of the first settlements; during the middle decades of the nineteenth century, however, large waterpower complexes mushroomed throughout the United States, the best known at Lowell, Massachusetts. Historians should not view these complexes as the prelude to Pittsburgh and Detroit; they were high technology culminating an era.

Because he has not done a social history of economic and technological change, Hunter's book will disappoint some historians. Instead, he sees history through the eyes of rural mill owners, millwrights, hydraulic engineers, and manufacturers of mill equipment. He and they tend to focus upon the technical and economic context instead of on

the social. His interest does not extend to workers, nor does it emphasize large-visioned entrepreneurs. Therefore, we learn, for example, of the technical and economic reasons why the turbine replaced the water wheel, and we are instructed about the difference in the empirically designed mixed-flow turbine of American inventors and engineers and the more theoretically based turbine technology of the formally trained European engineers. Hunter's subtle analysis of the technical-economic complex results in part from his choice of sources: generally, he relies upon contemporary economic and engineering reports and periodicals.

Historians will undoubtedly find reading the six-hundred-page Hunter study like taking a drink from a fire hydrant—not easy to absorb. As a matter of fact, even Hunter does not have his material completely under control. Despite their emphasis upon economic and technological history, the themes and narrative components are too diverse to be readily organized. Hunter's overall organizational structure is not sufficiently complex or embracing to provide the unity and coherence found in his *Steamboats on the Western Rivers* (1949), which was, however, a less challenging organizational problem. As a result, the thematic structure of *Waterpower* is not as striking.

Within the chapters, by contrast, Hunter usually has control. For instance, the chapter about "the decline of direct-drive waterpower" offers a detailed and memorable explanation for the paradox of the decline of waterpower at the time of its richest technological development. Hunter sees that the technology was ripe and sophisticated but inappropriate after 1870 as the engine of industrialization. The large city had become the site of rapid industrialization, and steam, not water, had survival power in that environment.

On the other hand, one must ask why Hunter gave an entire chapter to the transmission of power through millwork, and why he had to pile up—to the point of triviality—instances of the first industrial cities, such as those in the South. The answer probably lies in his conviction that he can prove his hypotheses by amassing facts. By contrast, the great strength of this admirable study is the brilliant insight of the author. His hypotheses have all the hallmarks of seminal theory. I hope that Hunter will leave much of the spade work to others as he opens new vistas on American history in the two additional volumes in progress, one on steam, and the other on the "transmission revolution" wrought chiefly by electricity.

THOMAS P. HUGHES
University of Pennsylvania

MICHAEL FELDBERG. *The Turbulent Era: Riot and Disorder in Jacksonian America*. New York: Oxford University Press. 1980. Pp. vi, 136. Cloth \$8.95, paper \$2.95.

The Turbulent Era is a slim volume that describes and attempts to explain violence and rioting in the United States between 1830 and 1860. The author gives inordinate attention to events in Philadelphia because, as he frankly concedes, he has previously focused his primary research on that city. Since the book was written for a "general audience," it almost entirely eschews footnotes, its bibliographical essay of less than two pages has "omitted the titles of scholarly articles because of their relative inaccessibility to those not on college campuses," and its literary style and level of analysis, while almost always clear, are typically simple and at times simplistic.

Although Michael Feldberg is a knowledgeable and thoughtful student of antebellum rioting, it is doubtful that his new book will succeed with its intended audience. The descriptions of riots that fill most of the pages are thin and uninspired, competently written at best but lacking in the interest that the topic deserves. Furthermore, the book's analyses are separated from its narrative. While these analyses are usually sensible, they are neither profound nor intellectually stimulating and at times lapse into banality. Feldberg advises that "groups may choose violent conflict, but such behavior is not required" (p. 85), and he closes the book with the prediction that violence and rioting are likely to continue "so long as groups continue to conflict, and so long as violence remains a part of the nation's political process" (p. 129). Further detracting from the book's chances with a literate audience are too many reminders that riots give "expression" or "concrete form" to this or that feeling, that "such was the consequence paid [*sic*]" by a particular group, and that certain types of riot were "numerically the most common" (p. 43).

Not that the book is valueless to scholars. Feldberg's categorizations of riots are for the most part sensible, and his analysis of their causes is lighted up by an occasional flash of insight, as in his discussion of the relationship between violence and either the public behavior of Andrew Jackson or the growing cult of the common man. Feldberg is aware that his discussion of causation is subjective and speculative, that the same facts could be differently interpreted. The great problem with the book to historians is its excessive number of strikingly dubious judgments, whether about the motives and the behavior of rioters or the consequences of their behavior; downright error, whether about the goals of abolitionists, the social structure, the aspirations of working men, or the activities of their organizations; and the thinness and obviousness of much of its argument.

EDWARD PESSEN
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IAN R. TYRRELL. *Sobering Up: From Temperance to Prohibition in Antebellum America, 1800-1860*. (Contributions in American History, number 82.) Westport, Conn.: Greenwood Press. 1979. Pp. xii, 350. \$23.95.

For years alcohol was more apparent at academic gatherings than in scholarly monographs. Recently, however, there has been much historical discussion of it as an item of consumption, a political issue, and part of a general program of reform. Although Ian R. Tyrrell takes this diverse interest into account, he intends *Sobering Up* to be what we have long lacked: an up-to-date, reasonably dispassionate, social analysis of the temperance movement prior to the Civil War. By that standard, its accomplishments are genuine and its failures partial.

Tyrrell regards temperance activity before the mid-1820s as a "false start," dominated by a Federalist elite committed to preserving its authority and exerting "social control" over the lower orders. With formation of the American Temperance Society in 1826, the crusade began in earnest. It gained an attractive leadership, a broad constituency, and effective tactics.

The ATS spoke for "improvers." Far from being provincial reactionaries, these men and women represented the most dynamic elements of American capitalism. Although their program was "self improvement," not social control, their actions paint for Tyrrell a "puzzling picture of men who had succeeded economically through personal effort yet rejected a self-help philosophy" by calling on the government to ban alcohol (p. 128). Their crusade took a detour in 1840 as the newly founded Washingtonian movement drew antiliquor strength and tactics from artisan life. By the decade's end, however, Washingtonian societies were either in shambles or dominated by more respectable folk like those who ran the ATS.

In this telling of the story, temperance had coherence and consistency from 1826 to 1860. It was a class-based response to drunkenness, pauperism, and crime. Its goal was not so much repression as what reformers regarded to be social and individual progress. Its tactics developed logically from early antiliquor license campaigns to the push for prohibition in the 1850s. In spite of the Washingtonian interlude, temperance remained in the hands of confident entrepreneurs and their clerical allies.

As social analysis, Tyrrell's interpretation has weaknesses. It is most convincing in the close study of Worcester, Massachusetts, which the author uses as a model for temperance in the Northeast. It is less effective when he extends it to rural areas and, without much evidence, to the South. Indeed, Southern temperance gets short shrift, allegedly because there was so little of it and so few "improvers" to promote it. (My hunch is that there is more to

Southern and Western temperance than Tyrrell lets on and that some of the problem is the Northern bias of national organization records.)

For someone who insists on the primacy of "property and class" in explaining prohibition, Tyrrell is remarkably loose in his terminology. His "improvers," for example, include farmers whose wealth did not differ from their antitemperance neighbors but whom he characterizes as more enthusiastic about commercial agriculture. Contrary to Tyrrell, consciousness as much as class seems significant here. Elsewhere, Tyrrell's language is too imprecise to describe the complex social reality he has discovered. He once, with exaggeration, labels temperance leadership as "upwardly mobile captains of industry" (p. 167). He calls the Washingtonians "lowly" or "humble" artisans while denying that they were "social dregs." That implies more of a social analysis than appears in the text, and it is using a rhetorical blunt instrument rather than scalpel to define groups and their relations to each other. Both artisans and elites were ill-formed as classes and internally divided. Temperance was bound up with conflicts over life-styles and values that cut across these shifting lines of class, property, and occupation.

Tyrrell's attempt to relate temperance ideas to class is likewise suspect. From Merrill D. Petersen he borrows a definition of ideology, including the notion that it is a "gross oversimplification" (p. 134). There are more sophisticated definitions available. But Tyrrell's heart is not in assessing the beliefs of temperance men and women. These are largely relegated to the shortest and sketchiest chapter—a pity because his instincts are right when he connects reformers' views of progress to material conditions and when he argues that the role of evangelical Protestantism has been exaggerated. Those are suggestions worth more extended discussion.

Although Tyrrell's analysis is neither entirely new nor persuasive, it is valuable in part because it sensibly loosens linkages that have been fastened too tightly between temperance and Whiggery, evangelical Protestantism, and nativism. What remains is to provide a more systematic study of temperance's appeal, both in the Northeast and elsewhere, and to show in a more cogent way how culture interacted with social and economic forces to produce a particular perception of alcohol.

RONALD G. WALTERS
Johns Hopkins University

BRAD AGNEW. *Fort Gibson: Terminal on the Trail of Tears*. Norman: University of Oklahoma Press. 1980. Pp. xi, 274. \$14.95.

Established on the banks of the Grand River in 1824 in what is now Oklahoma, Fort Gibson played

an important role during the era of Indian removal. The initial task of Matthew Arbuckle, who located the post and served as its commander until 1841, the terminal date for this study, was to maintain peaceful relations with nearby tribes and more particularly to prevent conflict between the Osages and early Cherokee immigrants. In ensuing years the garrison was involved in survey work, road construction, and escorting delegations of eastern tribes that arrived to investigate lands preparatory to removal. Establishment of relations with Plains tribes and prevention of conflict between them and immigrant tribes became one of the most important functions of the post.

While Arbuckle was not entirely successful in the latter activity, he did minimize such conflicts through mediation and prevented serious intertribal warfare. The army played an important function in assisting the resettlement of eastern tribes and provided a cultural buffer for tribes in Indian Territory, cushioning the culture shock for these people. Brad Agnew argues that the army at Fort Gibson represented Indian interests more conscientiously than any other frontier institution and that the commander was guided by the desire to avoid battle and to seek compromise.

The material collected by Agnew confirms Francis Paul Prucha's view that the army was as familiar with the broadax as with the bayonet and rejects descriptions of the frontier army as an aggressive institution that sought conflict with Indian tribes. While it is useful for that, the book might have been more effective as a biography of Arbuckle, which at times it appears to be. Moreover, while officers at the post engaged in important activities relating to Indian removal, the true significance of their actions does not come through, perhaps because of the nature of the documentary evidence and perhaps because these were primarily intertribal issues and documents reflecting tribal viewpoints do not exist. The result is the undramatic story of the everyday workings of a frontier post, and Agnew might have better accentuated the functions of the post by a reduction in length or by expanding coverage to 1857 when the fort was abandoned temporarily.

RICHARD N. ELLIS
University of New Mexico

DON E. FEHRENBACHER. *The South and Three Sectional Crises*. (Walter Lynwood Fleming Lectures in Southern History.) Baton Rouge: Louisiana State University Press. 1980. Pp. xii, 81. \$8.95.

Based on Don E. Fehrenbacher's 1978 Fleming Lectures at LSU, this short book explores a large theme: Southern secession. Following his mentor, Avery Craven, Fehrenbacher wants to separate un-

derlying causes for North-South conflict from the secession process itself and ask, given the strength of the former, why the latter occurred so late. Rejecting recent attempts at large-scale generalizations as modish but unilluminating, he suggests that the key is not in asking new questions but phrasing old ones "more carefully" and pursuing "answers more diligently and rigorously" (p. 4). His central theme, really metaphor, suggests that an explosive charge capable of creating sectional conflict was always present. There was no particular increase in the size or explosiveness of that charge. But in the late fifties, the second component necessary, a fuse effective enough to set it off, was found.

In each of the three great sectional confrontations in Congress—1819, 1846–50, and after 1854—Fehrenbacher finds the charge: united Southern determination to defend slavery and the section's rights and place within the Union. Each crisis added something to the charge's potency, but secession never occurred although often threatened. The legislative process, with its built-in, moderating political structure, was not the place to build the fuse. Adventurism was always checked, even by small groups of compromisers. It was not until the situation was forced out of Congress into the electoral arena that the fuse was found, or to change the metaphor as Fehrenbacher does, "a different finger was [now] on the trigger mechanism. Control passed from the professional politician to the ordinary voter"—meaning those who would vote Republican (p. 54).

Fehrenbacher's interesting retelling of a familiar tale in a different framework has two prime virtues: his efforts to separate latent causes from the actual flash point when great events move to specific conclusions, and, in a day when much is made of cultural differences and irreconcilable social tensions leading to inevitable (irrepressible?) conflict, his emphasis on the political dimension of what was ultimately a political act. The metaphor, of course, can be presented differently. Rather than the explosive charge being always fully formed and potent, one can argue that the chemicals necessary to create the charge were, but they were not mixed together for some time due to differences among those on the scene. It is an important distinction. In this and other matters, the shortness of the book is frustrating, as Fehrenbacher recognizes. Much is undeveloped, complexities are smoothed over, nuances cannot be explored. Since the whole is a commentary on recent trends in the study of antebellum society and politics, such development and exploration seems especially pertinent. Still, we are left with a valuable argument about questions to ask and the focus needed to find answers to our most enduring puzzle.

JOEL H. SILBEY
Cornell University

WILLIAM GILLETTE. *Retreat From Reconstruction, 1869–1879*. Baton Rouge: Louisiana State University Press. 1979. Pp. xiv, 463. \$27.50.

In this volume William Gillette, of Rutgers University, provides the first detailed account of post-1868 national Reconstruction policy since those of James Ford Rhodes and John W. Burgess, which appeared at the turn of the century. His interpretation falls generally within the mainstream of Reconstruction historiography since 1960. He does not find Reconstruction to have been the radical, constitutionally questionable process described by Burgess and Rhodes, and of course he rejects their belief that the racial inferiority of black people doomed the program from the beginning. Rather, Gillette makes quite explicit his conviction that Reconstruction was not radical enough. Likewise, he is barely concerned with Republican corruption in the South or nation—a central concern in the earlier histories. In his chronicle, the freedmen and Southern Republicans emerge as victims, not oppressors. It follows that Gillette finds Rutherford B. Hayes to have been something other than the enlightened statesman of sectional reconciliation and reform portrayed by his biographers. He judges him an inept traitor to ideas of freedom and racial equality, clinging adamantly to the illusion that appeasement would build a successful Republican party in the South.

But Gillette does not repudiate all older judgments. He follows a trend discernible in recent Reconstruction historiography that questions the degree to which Northern Republicans were really committed to racial justice, suggesting an hypocrisy that earlier historians also charged against “the party of freedom.” He also shares earlier historians’ antipathy to Ulysses S. Grant, although for different reasons. Gillette’s criticism is not that Grant surrounded himself with corrupt political allies and used troops to oppress the South. Rather, Gillette condemns him for vacillation and failure to establish an overall Southern Republican strategy. Indeed, Gillette seems baffled by the twists and turns in Grant’s Southern policy. The Republican retreat from Reconstruction culminated in 1875, Gillette argues, during Grant’s administration rather than in 1877 with the famous Wormley House Bargain. Hayes’s abandonment of Southern Republicans merely confirmed an earlier policy.

Immense research has gone into this volume. The number of citations to manuscript sources is staggering. While most of the information is not new, some of it is, and the perspective, while a logical consequence of changed interpretations of events prior to 1869, needed articulation. It is an important book, issuing judgments with which many historians must reckon.

There are shortcomings, however. Gillette’s deci-

sion to organize his study more topically than chronologically often leads to confusion. More seriously, it makes it difficult to assess how developments in one area related to developments in another, and as a whole Gillette does not do it. Furthermore, Gillette never reconciles the contradiction between his conviction that Republicans’ weak commitment to racial justice played an important—perhaps the most important—role in the retreat from Reconstruction and his frequent suggestions that Reconstruction legislation improved party morale.

This makes Republican support of such legislation hard to understand, and Gillette’s explanations will probably leave many readers unconvinced. Finally, there is an imbalance in the work. Events in several Southern states are described in detail, but there is hardly any discussion of the growth of Liberal Republicanism and its consequences and just as little of the effect upon Reconstruction of controversy over other issues. Therefore, thorough as this study is in some respects, it leaves much still to be done.

MICHAEL LES BENEDICT
Ohio State University

LOIS W. BANNER. *Elizabeth Cady Stanton: A Radical for Woman’s Rights*. (Library of American Biography.) Boston: Little, Brown. 1980. Pp. xiii, 189. \$9.95.

This is a short, readable biography of a great figure in the nineteenth-century women’s rights movement. Elizabeth Cady Stanton, with her coworker Susan B. Anthony, was the prime mover and principal adornment of Victorian feminism. Yet she has never been easy to deal with. As a champion of freedom for women and slaves and as an advocate of divorce reform and other unpopular causes, she excites admiration. But Stanton also allied herself with the unsavory George Francis Train and the notorious Victoria Woodhull with unfortunate results, put women’s rights ahead of black rights in the perilous 1860s, and embarrassed suffragists by attacks on the clergy and the Bible. Too radical by nineteenth-century standards, she had little interest in economic problems and so was not radical enough by those of the twentieth.

These and other contradictions, products of a long, busy life, raise difficulties that cannot be resolved in a work of fewer than two hundred pages. Within these limits Lois W. Banner has been able to outline Stanton’s career, survey her writings, and offer new information about her private life. The hints in Stanton’s autobiography regarding her disappointments as wife and mother are explained, as are the tension between herself and Anthony. Banner views Stanton’s radicalism as genuine and crucial, and she agrees with Stanton’s opinion of the

suffrage movement in the 1890s as too cautious and narrow. She conveys a good sense of Stanton's lively, prickly character, though not of her eloquence and wit. These last, however, can only be gained through quotations of a length precluded by the book's format.

Much remains to be done. As Banner points out, Stanton still does not have a modern, full-dress, scholarly biography. Her place in American history has yet to be defined. A woman of many talents, she never developed any one to its fullest. A leader with few followers, a prolific writer whose corpus is less than the sum of its parts, an intellectual who relied more on impulse than disciplined thought, Stanton is hard to evaluate. The nation does not honor Stanton, having chosen instead to memorialize Susan B. Anthony though she was far less brilliant. Is this a mistaken judgment arising from Stanton's greater unconventionality in later years or a just response to her shortcomings as an organization woman? It is such questions that one hopes to see addressed someday. Until then Banner's work will serve as the best introduction to what was, at the very least, a quite remarkable life.

WILLIAM L. O'NEILL
Rutgers University

EDWARD H. MCKINLEY. *Marching to Glory: The History of the Salvation Army in the United States of America, 1880-1980*. San Francisco: Harper and Row. 1980. Pp. xviii, 286. \$8.95.

Edward H. McKinley writes with the enthusiasm of a recent convert about the first century of the Salvation Army in America, but the Army's outdated military jargon grates upon the ear. This is an old-fashioned institutional history, a narrowly conceived chronicle of the Army's struggles, triumphs, internal bickerings, and multifarious religious and philanthropic undertakings to win "captives" for Christ. McKinley makes only the barest effort to relate the denomination to the social and religious context of the past century and no effort whatsoever to compare and contrast its work to that of other religious and charitable institutions. His principle purpose is to correct the popular image of the Salvation Army as primarily a charitable organization when it is, and always has been, fundamentally an evangelical, soul-winning organization.

Among the strong points of the book are its frank acknowledgment of the autocratic nature of the Army and of the nepotism of the Booth family. McKinley is also frank about the failure of the organization to do much to help blacks or foreign-born Catholics and Jews. Some readers may be surprised to learn that the major thrust of the Army's rehabilitation program during the Progressive Era

was to take people out of the sin-filled cities and return them to farming. Except for its endorsement of Herbert Hoover in 1928, the Salvation Army has been apolitical. Believing wholeheartedly in the Protestant ethic, Army leaders have never been the least bit interested in social reform. This is a portrait of a thoroughly bourgeois organization.

The most original part of the chronicle is its description of the Salvation Army's decline since 1950. It has become a bureaucracy, recruiting most of its staff not from new converts but from descendants of old Salvation Army families. The Army has moved from a sect to a church, and it is mildly troubled by the acceptance of federal funds for many of its projects. Yet, like other fundamentalist denominations, it retains its old theology. McKinley thinks there is life in the old warhorse still, but all his evidence points the other way. This book provides little explanation for the Army's rise and fall, but it is full of colorful stories about its zealous and often eccentric leaders. Inadvertently, McKinley confirms George Bernard Shaw's view of the Army: its captives went to work for Christian capitalism.

WILLIAM G. MCLOUGHLIN
Brown University

JAMES E. FELL, JR. *Ores to Metals: The Rocky Mountain Smelting Industry*. Lincoln: University of Nebraska Press. 1979. Pp. xi, 341. \$21.50.

American historians of the mining frontier and of industrial development will welcome this deeply researched and fairly readable account of the rise, expansion, and ultimate decline of the nonferrous smelting industry that centered in Colorado after 1870. Front Range goldmining, which boomed tremendously following John H. Gregory's great strike below Central City in 1859, collapsed almost as precipitously when the district mines began hoisting complex and "refractory" ores. (The economic and financial aspects of the boom and collapse have been narrated in the recently published book, *A Mine to Make a Mine*, by Joseph E. King.) Like the Southern goldmining boom and collapse of 1838-45, the Colorado mines were producing ores whose assay values were high but whose values were technically impossible to recover without incurring prohibitive costs either of transportation or of state-of-the-art reduction.

Prosperity was restored when Nathaniel P. Hill established the Boston and Colorado smelter at Black Hawk, supervising development so that the Front Range mines again became profitable and the region again regained its economic impetus. Hill is therefore deservedly as much a local hero as was Gregory, yet the chronicle of Hill and of his sci-

entific and economic imitators and successors has not until now been well summarized. The volume works systematically through the rise of silver-lead smelting at Leadville, the growth of "valley" smelting at Denver, Pueblo, and Durango, and concludes with the saga of the Guggenheim family and its American Smelting and Refining Company (ASARCO). James E. Fell, Jr., has made a very important contribution to a critical but hitherto ignored chapter of both Western and industrial history.

The study is based upon the best available sources, ranging from archival materials to professional engineering papers. The presentation of the managerial and organizational growth of the smelting industry is thorough and admirable—perhaps somewhat dull reading at all times, but this is the fault of the protagonists, not of the author. Insofar as the book deals with corporate and economic history, it thoroughly deserves a AA-1 rating.

Unfortunately the same cannot be said of the author's handling of the equally central theme of metallurgical research and advancement. What could have been a fascinating account of scientific struggle is rather hastily and superficially mentioned, then dropped. It appears that Fell either preferred not to trouble the reader with technical information or felt unable to do it justice. The book, however, would have been vastly improved, striding forward on two legs instead of hopping along on one, had the author sought the editorial guidance of a competent metallurgist. In the end, the scientifically inclined reader has learned quite well what the Colorado smelting industry did but very little about how it did it.

The other major complaint must be laid at the door of the designer of the book. The one map is quite inadequate and the illustrations too few. Worst of all is the binding of the review copy received. It is a sickly, repulsive orange, marked (or marred) by spine stamping in an ugly, typewriter-like typeface. The book will unquestionably stand out upon a library shelf, but not for any reason of grace or attractiveness.

These strictures aside, *Ores to Metals* is strongly recommended to all interested parties as a valuable and scholarly contribution to a hitherto ignored chapter of our national development.

OTTIS E. YOUNG, JR.
Arizona State University,
Tempe

AUGUST W. GIEBELHAUS. *Business and Government in the Oil Industry: A Case Study of Sun Oil, 1876-1945*. (Industrial Development and the Social Fabric, number 5.) Greenwich, Conn.: JAI Press. 1980. Pp. xvi, 332. \$27.50.

For many decades, the petroleum industry has proved to be a magnet for historians. In undertaking this study, August W. Giebelhaus has set for himself two goals: a history of the Sun Oil Company through World War II and, more tightly focused, a case study of Sun's role in the struggle within the industry to shape public policy.

This family firm had its origins in the last quarter of the nineteenth century when its principal founder, Joseph N. Pew, Sr., built an enterprise that was able to operate successfully in the shadow of Standard Oil. Pew early implemented policies that would characterize his company: reliance chiefly on internal financing in order not to jeopardize family control; vertical integration; and a persistent striving for new strategies and innovations. As a small rival of Standard Oil, Pew also believed in vigorous enforcement of antitrust laws.

Following the discovery of huge oil fields in Texas and Oklahoma (in which Sun shared) and the dissolution of the Standard Oil Trust after 1911, the structure of the oil industry changed from domination by a single firm to oligopoly. The second generation of brothers and cousins kept faith with the policies of the founder in building Sun into a respected and highly efficient smaller major oil company. In exploring Sun's growth, Giebelhaus pays more attention to developments in transportation, refining, and marketing than in producing—perhaps the result of records being richer for the first three functions.

Giebelhaus also examines carefully the drive on the part of some major companies to maintain intra-industry cooperation after a successful (and profitable) experience during World War I and the drive to make government the regulator of crude oil production after geologic science had matured to that point where production could be controlled to reduce physical (and economic) waste. The first drive resulted in the American Petroleum Institute; the second in the Interstate Oil Compact and the Connally Hot Oil Act. While the Pews joined the API and ultimately became convinced of the wisdom of production restriction, they fought vigorously against any further intrusion of government authority except in wartime. They put their faith in free market forces as in the best interest of both industry and consumers. Today's shortages make their arguments seem archaic, but the efficiency with which they conducted their enterprise suggests that their position was not without merit in its time.

GERALD T. WHITE
University of California,
Irvine

JOHN E. SEMONCHE. *Charting the Future: The Supreme Court Responds to a Changing Society, 1890-1920*. (Con-

tributions in Legal Studies, number 5.) Westport, Conn: Greenwood Press. 1978. Pp. xiii, 470. \$25.00.

John Semonche has crafted a new genre of Supreme Court historiography. By focusing neither on dominating personalities nor conspicuous events but rather on the time span itself, he achieves a panorama and lineality yielding an appraisal of both men and events. Moreover it is an appraisal that might not be available in any other way.

Specifically, his time span is the thirty years between 1890 and 1920. The thesis distilled from the apperceptive mass is this—that notwithstanding the spectacular and (in Semonche's view) atypical pyrotechnics of *Pollock*, *Hammer*, and *Lochner*, the workaday work of the Supreme Court over those three decades was not reactionary either politically or psychologically but instead was creatively responsive in adapting the Constitution to new times. And even more surprising is the nuance that 1890–1920 was in fact the seedtime for the judicial imperialism of the Warren Court.

The specifics are arresting too. Sitting as a court of review on the existing record, the author is acute and persuasive in resolving the mystery of the vacillating justice in *Pollock* and points his finger at Stephen Field, who also comes through as a constitutional jurist of towering dimensions.

This sort of story simply does not tell itself, and in selecting the frame, Semonche weaves his tapestry with a masterful hand providing synthesis and integration through subtle changes of mood and pace and the use of concrete and interesting facts.

GERALD T. DUNNE

St. Louis University Law School

DANNEY GOBLE. *Progressive Oklahoma: The Making of a New Kind of State*. Norman: University of Oklahoma Press. 1980. Pp. xi, 276. \$14.95.

The purpose of *Progressive Oklahoma* is to demonstrate that Progressivism appeared at the state level before becoming a national cause. Danney Goble uses Oklahoma as a model because he regards the new state constitution of 1907 as a particularly lucid example of Progressive reform; Oklahoma's founding fathers were touted as creating not just a new state but a new kind of state. The author traces the Sooner State's transformation to a territory of the first class, covering the period from about 1887 to 1907, and casts his work in the context of early Progressivism.

Progressive Oklahoma examines the sources of the Oklahoma reform movement and explains its application. And it traces the rise of homesteader political activity that increasingly contained the tincture of protest that coalesced into Progressivism. In

opening this new frontier, Oklahoma's pioneers came face to face with the emerging and increasingly powerful business corporation. Commonly its officers were in league with local, state, and national politicians, which enabled them to evade their fair share of taxes, gouge the consumer, and generally prey on the public with impunity. The author points out that prices for essentials rose as much as 53 percent in the period 1897–1909. Oklahomans blamed trusts for these price increases. They were antagonized by the blatant corporate disregard for public health and safety and resented their growing dependence on these giant enterprises. Also, they were frustrated by their seeming inability to subject their corporate tormentors to any sort of control. Certainly they resented business combinations tampering with democratic processes.

Then *Progressive Oklahoma* demonstrates the growing public awareness of the remedies available to Sooner pioneers, the emerging Progressive reforms that included the direct primary and initiative and referendum, rated as the most effective tools for “the absolute elimination of all corporate influence and control from all political and legislative bodies” (p. 176).

These reforms were taken up by the Oklahoma Socialist Party, Oklahoma farm and labor organizations, rank and file Republicans and Democrats; even a few party leaders espoused the reforms including Territorial Governor Thompson B. Ferguson, an advocate of the direct nominating primary. The Democratic and Republican parties, however, were locked into the old politics of the Gilded Age.

The author shows that probably three-fourths of the delegates to the constitutional convention were elected less on a party platform and more on a general commitment “to kill monopoly . . . purify the ballot . . . restore to the people their natural rights” (p. 177). Thus the Oklahoma constitution became the focus of Progressive thought, containing provision for the long ballot, single term for governor, initiative and referendum, secret ballot, and direct primary. Its revolutionary character caused it to be rated across the nation as “the most radical organic law ever adopted” (p. 226).

The author has composed a readable account of the anomalous and complicated evolution of Oklahoma statehood, conceived in a Progressive ambience, and comprising a daring experiment in state making. And he presents a convincing case for his thesis that Progressivism appeared on the state level before moving into the national political arena.

ARRELL MORGAN GIBSON
University of Oklahoma

DOUGLAS HENRY DANIELS. *Pioneer Urbanites: A Social and Cultural History of Black San Francisco*. Philadelphia: Temple University Press. 1980. Pp. xx, 228. \$17.50.

The author gave himself the task of doing a social and cultural history of Afro-Americans in San Francisco for a period covering nearly a century after 1860. He does this with much original research in ten chapters preceded by a personal foreword written by Nathan I. Huggins, who lived in San Francisco in the 1940s.

Douglas Henry Daniels feels that too much of black history writing concentrates on either the most distinguished blacks or on the masses whose lives are described as deprived or lamentable. He wishes to focus the reader's attention on a less-noticed middle group whom he feels had a sense of personal worth that also deserves notice. His presentation includes pictures of this group taken by themselves and, of more importance, oral interviews for more recent history. This study also dwells on the reasons for the failure of black San Francisco to grow as rapidly as other black communities in California in the century after the Civil War.

The story of the obstacles that the union movement placed on black job opportunity is presented, and the author illustrates this with the case of the famed Palace Hotel where an almost all-black staff in the 1870s was replaced by an almost all-white staff by the turn of the century. Daniels also makes some striking comparisons between the Negro of San Francisco before World War II and the new black arrivals of the World War period. He discusses the culture shock and even some of the resentment of the older group toward the newer one.

There are writing and organizational problems in this book, however. The one-word chapter headings are inadequate clues to the contents. It would be laborious to deal with each title, but "Pioneers" were not all pioneers, "Scouts" were not literally all scouts, and some other titles like "Survivors" and "Cosmopolites" are more poetic as designations than historical. The laconic titles stand in contrast to the too often rambling and wordy text. The uninformed reader will be disconcerted by adjacent illustrations that leap decades in time from the nineteenth to the twentieth century. This reviewer was puzzled not to find Reverend Jeremiah B. Sanderson in the chapter on "Leaders" while other personalities in the book are sometimes larger than life.

Perhaps source limitations explain the minimum of church history for the post-Civil War period, although church decor is noted by the author. Research is still needed on the California phase of an interesting but failed effort to unify the two African American Methodist Episcopal churches. And did not Daniels's group, which emulated much of white life in its efforts at respectability, have any position during the prohibition debate?

Yet this work makes a useful contribution. A hitherto neglected time period in California Afro-American history has finally received scholarly at-

tention and will perhaps stimulate further investigation.

RUDOLPH M. LAPP
College of San Mateo

JUDD KAHN. *Imperial San Francisco: Politics and Planning in an American City, 1897-1906*. Lincoln: University of Nebraska Press. 1979. Pp. 263. \$17.95.

Judd Kahn's *Imperial San Francisco*, despite a partially misleading title—for in neither form nor function did the city realize its "imperial" aspirations—amply documents the difficulties of achieving comprehensive city planning in an already settled and democratically governed community. It also provides abundant insights into the frustration, ambivalence, and conflict of interests inherent in urban public policy making. It is a thoughtful, although occasionally repetitious, work.

By happenstance, less than a year before the earthquake and fire of mid-April 1906, San Francisco's board of supervisors had accepted a comprehensive plan that could have produced from the ashes of the stricken city a "new San Francisco" of beauty and order. The plan was the work of Daniel Burnham, fostered by patrician former mayor James D. Phelan and a board of supervisors dominated by merchants and professionals. A blend of beauty and utility governed Phelan's thinking about a comprehensively planned San Francisco. A beautiful and well-ordered city would produce orderly citizens; it also would enhance the city's potential as a resort center and secure its position as the capital of a developing commercial empire in the Pacific. As part of his plan, Burnham proposed the widening and relocation of streets and the provision of new centers for civic, financial, and industrial activities. Phelan hoped that these changes could be achieved voluntarily by a citizenry in which loyalty to place would support expanded municipal activity within the context of the community's traditional faith in private enterprise.

Contrary to what one might have expected, the devastation of 1906 hindered rather than helped the implementation of the Burnham plan. The desire to achieve a speedy return to normalcy discouraged support for extensive physical changes lest they uproot former business locations and occasion delays resulting from consequent litigation. The obsession with immediate rehabilitation released a hornet's nest of interest conflict. Transit operators argued the merits of overhead trolleys versus underground conduits for streetcar propulsion; local capitalists criticized the intrusion of "outside" Eastern investors; real estate firms opposed municipal housing of even minimal standards for the homeless; and Phelan and his friends resisted the activities of the

board of supervisors, now largely labor oriented, dominated by "Boss" Abraham Ruef, and suffering from an image of indulgence in bribery and corruption. Ever present was the city's concern for safeguarding private enterprise—a consequence, perhaps, of its relative youth and the nature of its historical development. Ironically, it was Ruef who maneuvered through the state legislature a vestige of the Burnham plan, a viable means of achieving street widening (without Phelan's help through fear that the enforcement would be carried out to the advantage of a demonstrably corrupt political machine), only to have it defeated by popular vote. In Burnham, San Francisco had its Baron Haussman, but the community proved to be no substitute for Napoleon III.

BAYRD STILL
New York University

JAMES D. STARTT. *Journalism's Unofficial Ambassador: A Biography of Edward Price Bell, 1869–1943*. Athens: Ohio University Press. 1979. Pp. xiii, 260. \$15.95.

Viewed from a perspective of almost forty years after his death, Edward Price Bell seems rather a sad figure. He enjoyed great prominence in the first quarter of the century as head of the foreign news service of the *Chicago Daily News* and as a roving correspondent interviewing world leaders. But even at the peak of his fame in the 1920s his journalism was becoming out-of-date in pushing causes (world peace, in particular) rather than attempting to get at the facts. In 1931 he had a falling out with Frank Knox, who had just taken over as publisher of the *Daily News*, and resigned from the paper. There were no other jobs to go to in that depression year, nor, as it turned out, could he find a market any longer for his free-lance writing. As war loomed he became progressively embittered—an American-Firster and rabid Roosevelt hater. By the time of his death in 1943 he had been forgotten. Today his name scarcely appears in the literature on journalism.

As an attempt to redeem a fallen reputation, James D. Startt's study is only a partial success. Bell does not emerge from these pages as a full-blooded personality, nor does the reader discover what he really believed in other than platitudes about peace and understanding between nations and "knowledge, courage, brilliance and integrity"—no small order—as the qualities for success in journalism. (Startt mentions, for example, that Bell was accused of antisemitism in his later years but does not explain why.) Occasionally the author falls into the trap of ascribing more influence to his subject than common sense justifies. To cite just one instance, accepting that viewpoints differ on whether the press

can ever actually change opinion or only reflect it, almost certainly Startt exaggerates when he writes of Bell's activity during a time of Anglo-American strain in the 1920s that "by means of personal interviews with British leaders . . . and by publishing articles in [several newspapers] he continued to remold British sentiment." One wishes as well that the author had been less prone to clichés and effusive use of adjectives. Cities "throb" in this narrative, vitality is "lively" (what else?), and in a single sentence on Bell's marriage we get "beloved partner," "affectionate mate," and "tender nature," which is simply too much.

But to harp only on such points would be wrong, because there is also much to admire in the book. The scholarship is exemplary, involving research in twenty-three manuscript collections in the United States and Great Britain. More important, Startt is impeccably fair in the way he presents the evidence and evaluates opposing points of view. He sees Bell as a major figure in twentieth-century journalism who has been unjustly neglected. He may be right. What matters is that he allows the reader leeway to arrive at a somewhat more modest judgment.

GEORGE JUERGENS
Indiana University,
Bloomington

WILLIAM M. HALSEY. *The Survival of American Innocence: Catholicism in an Era of Disillusionment, 1920–1940*. (Notre Dame Studies in American Catholicism, number 2.) Notre Dame: University of Notre Dame Press. 1980. Pp. xv, 230. \$16.95.

Scholars have long accepted the argument that the heart of American Catholic history lies in the effort to reconcile conflicting religious and cultural allegiances. In the late nineteenth century, progressive Catholic attempts to integrate Catholic and American values led to the condemnation of "Americanism" by Pope Leo XIII in 1899. When this was followed within a decade by the repression of modernism and associated theological speculation, the public dialogue about Catholics and America seemed to end. In fact, as William M. Halsey convincingly demonstrates, the need to define an identity at once Catholic and American was too profoundly felt to disappear; it merely took on new forms and found new expressions that reflected the changed situation created by the condemnations. In the interwar years, Catholic intellectuals found in the preservation of American innocence, by means of now-accepted Catholic philosophy, grounds for claiming both a fully loyal Catholicity and a completely respectable Americanism.

In his remarkably detailed account of Catholic culture in the years following World War I, Halsey

discovers that Catholics saw a unique opportunity in the crisis of progressive values associated with the "end of American innocence." Progress, freedom, creativity, and, especially, optimism were threatened by the cynicism and disillusionment that characterized American culture in the twenties. These values found a new foundation in neo-Thomism and new and unexpected supporters in Catholic cultural circles. The political values of the founding fathers and the cultural values of the genteel tradition, Catholics now claimed, had never been securely based in either the Enlightenment or romanticism. Instead, their origins lay in the civilization of the Catholic Middle Ages and would now experience a rebirth in the Catholic literary and cultural revival. Catholic philosophy, theology, literature, and art, reinvigorated by the apparent retreat of secular liberalism, would demonstrate the value and strength of the church and at the same time allow the church and its people to "re-Americanize America." Most importantly, they would define the form and limits of the Catholic subculture in such a way as to securely unite American Catholicism to the universal church but risk none of its hard-won respectability in American society. Even in the midst of the Great Depression, Catholics could argue for moderate reforms that were both Catholic and American, and they could claim with ever greater assurance that they had the answers to the problems of their country and their age.

Few listened, Halsey notes, because Catholics constructed this position without confronting the "poisons" of modern life that had led their non-Catholic counterparts to question and sometimes abandon the values of the nineteenth century. Only by isolating themselves from the realities of modern culture could Catholics remain secure in their innocence. Assurance was good for Catholic morale but a shaky basis for apostolic enterprise. In fact, Catholics were far less interested in "re-Americanizing America" than in securing their own position in the church and in American society.

Halsey's valuable study is one of the first serious examinations of Catholic thought in twentieth-century America. It will be most valuable to all students of American culture and to those interested in the background of the present ferment in American religion and in the Catholic Church.

DAVID J. O'BRIEN
Holy Cross College

RICHARD H. KING. *A Southern Renaissance: The Cultural Awakening of the American South, 1930-1955*. New York: Oxford University Press. 1980. P. xi, 350. \$15.95.

Richard H. King's study of the "Southern renaissance" attempts to trace "in a rather small group of

Southern writers and intellectuals between 1930 and 1955 a progress in self-consciousness" (p. 287). King examines the works of social scientists, historians, novelists, and essayists, tying them together on the basis of a common effort to confront Southern tradition. According to King, this effort produced responses ranging from the Agrarians' nostalgia for an idealized Old South to an attempt by other writers to "demystify" and transcend history's influence on their lives and on the region.

A Southern Renaissance is not easy to read. Part of the reason lies in King's writing, which is both repetitious and unfocused, and part in a conceptual fuzziness that marks many of his arguments. It is not even clear, for example, why he has chosen to include the particular writers he examines in his study. Still, King does show that coming to terms with history was a central theme in Southern letters during the period, and, although the idea is hardly new, he takes it in some interesting directions.

He is less successful, however, in showing any distinct process involving "self-consciousness" between 1930 and 1955. This may be because he devotes most of his effort to analyses of texts, giving inadequate attention to historical and literary evidence relevant to questions of development in Southern thought. Indeed, anyone interested in the relationships of the Southern renaissance to broader issues in American history and culture must look to other studies.

In addition, King is not entirely consistent in talking about a Southern tradition, the common factor in terms of which writers are to be discussed. At some points he emphasizes what he calls, after Sigmund Freud, the Southern "family romance," an understanding of Southern history structured by an image of family relationships. This serves him especially well in his analyses of racial ideas in the works of William Faulkner and Lillian Smith. In regard to other writers, however, the "family romance" either seems less relevant or is virtually forgotten as vaguer notions of tradition take its place.

A Southern Renaissance is, then, an uneven book. King often provides valuable insight into individual works by Southern writers of the time, but the book's lack of focus lessens its significance for an understanding of this important intellectual and cultural episode in American history.

DICKSON D. BRUCE, JR.
University of California,
Irvine

RICHARD POLENBERG. *One Nation Divisible: Class, Race, and Ethnicity in the United States Since 1938*. New York: Viking. 1980. Pp. 363. \$12.95.

Richard Polenberg's succinct, smoothly written overview of forty years in American history begins

with a mordant description of racial, class, and ethnic divisions on the eve of World War II. More than 30 percent of the nation's dwelling units lacked running water, inside toilets, bathtubs, or showers; 58 percent had no central heating. Intermarriage between blacks and whites was illegal in most states. Immigrants and their families (first and second generation) comprised more than a quarter of the total population, and foreign languages still dominated a great many urban and rural neighborhoods. By the late 1970s these conditions were gone. The country looked more homogeneous. Yet racial, class, and ethnic divisions remain deeply etched in American society. How those divisions manifested themselves during the intervening decades is the theme of the book, although not its only subject. Instead of attempting a systematic analysis of the shifting fault lines in American society, Polenberg has written a survey of recent American history that highlights the struggle for national unity. Every judgment is carefully modulated; no argument is pressed very far. Nevertheless, within this genial synthesis there is a guiding interpretation.

The interpretation is cyclical. Tendencies toward unity arise, make some headway, then splinter against divisive forces or situations; these in turn are checked by new centrifugal impulses, and so on. World War II created a general desire for greater national unity while reducing class and ethnic differences and mixing people together as never before. In the next fifteen years the Cold War and the great outpouring of city dwellers into the suburbs had complex effects that do not fit any clear pattern, but Polenberg's interpretation evokes a predominant impression of divisiveness. Then, as international tensions relaxed in the early 1960s, a great surge of reform (civil rights, war on poverty, immigration legislation) expressed a renewed determination to bridge the distance between races, classes, and ethnic groups. Finally, the Vietnam war pulled the country apart again by arousing class antagonisms, racial alienation, and compensatory ethnic pride. The Nixon administration, by deliberately exploiting these antagonisms, perpetuated them.

Without quite saying so, Polenberg assumes that racial and class differences are bad and that they tend to rise and fall together. The first proposition is intrinsically ambiguous: to the author, difference sometimes means social distance; at other times it means animosity, which is not the same thing at all. The second proposition, correlating racial and class differences, works better for some situations than for others, but it has the great merit of focusing attention on a fundamental relationship American historians have outrageously neglected. A more serious problem, rooted I suspect in the author's liberal bias, is a grossly disproportionate emphasis on the role of government in shaping American society. In

these pages social unity is advanced when federal policy points in that direction; it is set back when government does the wrong thing. In spite of this heavy overlay of political determinism, however, *One Nation Divisible* is the most useful and reliable social history yet written on the United States in the middle decades of the twentieth century.

JOHN HIGHAM

Johns Hopkins University

JOHN MORTON BLUM. *The Progressive Presidents: Roosevelt, Wilson, Roosevelt, Johnson*. New York: W. W. Norton. 1980. Pp. 221. \$11.95.

The Progressive Presidents is essentially a reaffirmation of belief in twentieth-century liberalism by one of its most distinguished and perceptive analysts. While acknowledging that their record is replete with paradoxes and ambiguities and the nation beset with a massive agenda of unsolved problems, Blum still pronounces the legacy of the progressive presidents as meritorious and seeks to dispel arguments raised by critics of either the left or the right. Despite the author's balanced approach and reasonable tone, his arguments primarily serve to reassure liberals, while doing little to persuade conservatives or radicals.

Not that Blum's work is a mere apologia for liberalism. He faults the first Roosevelt for leaving an "impressive but inconclusive record" (p. 59), while Wilson is charged with exhibiting the "ambiguities of reform" (p. 61). Blum acknowledges that Franklin Roosevelt suffered from a "problem of priorities" (p. 113) that was all too frequently resolved by trying to do everything at once and lays much of the blame for the failure of Lyndon Johnson's Great Society on the Texan's imperious administrative style and his egotistical intolerance of legitimate dissent. Moreover, Blum charges that all four progressive presidents occasionally misused power, faltered in guiding social change, stumbled in international ventures, and left their posterity a plethora of dangerous social ills. They erred most commonly and most grievously when international affairs "persuaded them arbitrarily to invade the legitimate rights of other nations or the individual rights of Americans" (p. 208).

Despite these significant reservations, the author concludes that the progressive presidents promoted the general welfare and provided the direction for improvement of the quality of national life. Their biggest achievement was to "sustain the American system by improving it" (p. 208). Even though frankly illustrating the manner in which liberalism suffers from its own inherent contradictions, Blum still assigns the major blame for the frustration of its goals to conservative presidents and congresses who

ordered strategic retreats and contends that radicalism has failed to gain a greater foothold partly because the progressive presidents accomplished as much as they did.

There is little in Blum's analysis to comfort or convince critics on either side of the vital center. There is little concession to the demands for less government or for a more radical socioeconomic order. Blum notes that conservatives have accused liberals of inventing false issues in order to aggrandize the federal government and themselves and that radicals had charged them with being unwitting tools of corporate capitalism, but he dismisses both these claims with little comment. The current crisis of liberalism is attributed primarily to a misinterpretation and a misrepresentation of its principles. Like the progressive presidents, Blum's purpose is "not to scuttle a discredited system," but "to restore it to operable condition" (p. 19).

JOHN D. BUENKER
University of Wisconsin,
Parkside

ALLAN J. LICHTMAN. *Prejudice and the Old Politics: The Presidential Election of 1928*. Chapel Hill: University of North Carolina Press. 1979. Pp. xiii, 366. \$20.00.

Allan J. Lichtman's study of the presidential election of 1928 is an ambitious and sophisticated effort to combine statistical analysis with reflections on the ideas and desires that moved the electorate. The author has kept up with literature about the election almost to the month of publication; about the only omission is several manuscript collections containing juicy materials on the campaign, among them those of Hubert Work, Lillian Wald, and Thomas B. Love. The core of the method is statistical. Lichtman speaks with authority the language of regression coefficients, interactive components, additive models, contextual effects, and multiple deviation analysis. He makes a strong case against the theory that the 1928 contest was a critical election, making for long-term shifts in popular alignments. His book should be read in conjunction with Kari Anderson's recent study of the same period.

Like others in our profession, I am not always comfortable with advanced quantitative methods; I confess to apprehension at a sentence like: "By transforming all variables of a multiplicative model into their logarithms (for example, $\log \gamma = \log a + b_1(\log X_1) + b_2(\log X_2)$), with suitable adjustment for values of zero, this mutually dependent model can be set forth in linear, additive form." Historians have in the past been too quick to endorse the conclusions of highly quantitative work that they do not fully understand. In Lichtman's case, the meth-

odology is generally clear and appears to make excellent sense. But if the use of simple quantitative techniques can sometimes trick us, then the employment of dozens of complicated tables, charts, and graphs can become exceedingly elusive. Studies such as Lichtman's should perhaps inspire respectful distance, allowing the ultimate test to be whether they survive the years of further scrutiny that other quantitative historians will provide. My common sense feelings about the election concur with conclusions to which Lichtman's statistics bring him: that while Al Smith's Catholicism did influence the election strongly, the vote was over more than the simple cultural issue pitting old-stock against immigrant-stock Americans and country convictions against city ways. I would propose, however, that much of the rhetoric of nomination politics within the Democratic Party in 1924 reflected precisely that stark cultural cleavage. And Smith's strong attempt to win the nomination in that year, along with his candidacy in 1928, did indicate the elevation to presidential politics of an urban, immigrant, non-Protestant electorate that had never before gained quite such symbolic prominence.

In bringing frank moral judgments to his work, Lichtman gives his study a fullness that quantitative analysis alone could not have attained. Here he invites a number of quarrels. He faults Herbert Hoover for not speaking out more often against the intolerance that Al Smith faced. But Hoover during the campaign did publicly and repeatedly denounce bigotry and instructed his party not to encourage it. How much of Hoover's campaign should have gone to helping out his rival on an issue that the Democrats themselves did not address firmly and sensitively? Economic progress, Lichtman says, was "painfully limited" and "the postwar spurt in real wages occurred primarily between 1919 and 1921 (an increase of 25.5 percent). Between 1921 and 1928, wages increased only 6.4 percent and real wages only 10.6 percent." That is a fairly quibbling "only." Lichtman, I believe, sometimes fails to judge the election by terms that the era itself would have recognized; nor does he quite take the opposite and perhaps equally legitimate course of judging it frankly from the outside and to the left of it. But studies as tight and revealing as Lichtman's can help us to our own judgments of this curiously benign and callous era.

DAVID BURNER
State University of New York,
Stony Brook

MICHAEL R. BESCHLOSS. *Kennedy and Roosevelt: The Uneasy Alliance*. New York: W. W. Norton. 1980. Pp. 318. \$14.95.

Joseph Kennedy and Franklin Roosevelt. The alliance between businessman and politician was not only uneasy. It was unnatural.

They never really liked each other. Kennedy, son of East Boston's immigrant Irish who make it big in Wall Street, used Roosevelt to gain respectability through public service. Roosevelt, scion of a distinguished Hudson River family, tolerated Kennedy's incredible cheek because he was a rare breed, a big businessman for Roosevelt.

Kennedy privately attacked Roosevelt "knowing these jeremiads would drift back to the White House" (p. 112). But he parlayed his public support into important positions: chairmanship of the Securities and Exchange Commission (1934); chairmanship of the Maritime Commission (1937); and, finally, ambassador to England (1938). Roosevelt defended the appointment to the Securities and Exchange Commission as "set a thief to catch a thief" (p. 88). And appointment of an Irishman to the Court of St. James was "a great joke, the greatest joke in the world" (p. 157).

When war came, Kennedy reasoned that England would lose. While Roosevelt moved ever closer to intervention, Kennedy urged England to make the best possible deal with Hitler.

Kennedy came home in 1940, determined to oppose Roosevelt's re-election. But he emerged from his first meeting with the president after his return singing his praises. Years later he explained that they had made a deal. Kennedy would support Roosevelt in 1940; Roosevelt would support Kennedy's son for governor of Massachusetts in 1942.

But by the end of 1940, the break was final. Kennedy's public statements earned him a summons to Hyde Park where, after less than ten minutes, Roosevelt asked Kennedy to step out of the room. He called Eleanor in: "I never want to see that son of a bitch again as long as I live. Take his resignation and get him out of here" (p. 229). Before the end of the war, Kennedy talked of a book exposing "that son of a bitch that killed my son Joe" and proving "the betrayal of the American people by Franklin D. Roosevelt" (p. 253).

Michael R. Beschloss has written a brisk story; the prose style is crisp; the research is thorough and impressive. The book, while not so important as some other books of the same genre like Robert Sherwood, *Roosevelt and Hopkins* (1950) or Joseph Lash, *Roosevelt and Churchill* (1976), is nonetheless a good book, a very good book that adds to the New Deal mosaic. Here is a case study of the lust for power vanquishing morality and idealism in which both men emerge badly discredited.

GEORGE WOLFSKILL
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ELMER L. PURYEAR. *Graham A. Barden: Conservative Carolina Congressman*. Buies Creek, N. C.: Campbell University Press. 1979. Pp. ix, 235. \$8.95.

Graham A. Barden, in this biography, emerges as the prototype of the conservative Southern Democrat who came to Washington on the crest of Franklin D. Roosevelt's New Deal. He heartily endorsed the president's sweeping economic and social legislation but became increasingly alarmed and disturbed over the rapid bureaucratization and socialization of the national government, its inflationary policies, and its violations of the Constitution and invasion of states rights.

As "Conservative Carolina Congressman," who represented North Carolina's Third District from 1934 to 1960, he not only performed yeoman service for his constituency, but also attracted national attention as member and long-time chairman of the House Education and Labor Committee. As such, he moved from an ardent supporter of the Wagner Act and the Fair Labor Standard Act to a position of hostility toward organized labor and from an ardent champion of federal aid to the states for education to a hostile opponent of federally controlled aid to education, which he felt would lead to almost constant federal interference in state and local school administration.

The author, after a scholarly and detailed run-down and analysis of Barden's congressional stands, weighs objectively his positive and negative influence on legislation. On the positive side, he secured sixteen million dollars for local improvements of rivers and harbors for his district and acquired three permanent military establishments: Camp Lejeune (marine barracks), Camp Davis (an anti-aircraft base), and Cherry Point (a marine aviation base). On the national level he was proudest of the Barden-LaFollette Act for rehabilitation of the physically handicapped and the George-Barden Act, which provided federal assistance for vocational training. He conceived the idea and was largely responsible for the Library Service Act and several acts providing federal aid to education in federally impacted areas. Instrumental in the passage of the Taft-Hartley Act, he was the author of section 14(b), which allows states to pass right-to-work laws.

Perhaps the effectiveness with which he blocked legislation was an even greater achievement. Barden himself said that what he kept from happening was far more important than what he caused to happen and that he "never knew the Republic to be endangered by a bill that was not passed" (p. 227). Two cases are in point: federal aid to education and the common site picketing bill.

It is to be hoped that Puryear's biography will

lead to similar books on Barden's North Carolina congressional contemporaries.

BLACKWELL P. ROBINSON
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GRAHAM D. TAYLOR. *The New Deal and American Indian Tribalism: The Administration of the Indian Reorganization Act, 1934-45*. Lincoln: University of Nebraska Press. 1980. Pp. xiii, 203. \$14.50.

In this book Graham Taylor examines government-Indian relations during John Collier's tenure as Commissioner of Indian Affairs. While he builds on previous work by Lawrence Kelly, Donald Parman, and Kenneth Philp, his purposes are different from those of these historians. Rather than examining the experiences of a specific tribe or Collier's personality and program, Taylor attempts to describe the implementation of the Indian Reorganization Act (1934), the most important statute of the Indian New Deal. His chapters of the background and design of the act cover well-known territory. Other chapters—on administrative changes in the Bureau of Indian Affairs, problems encountered in organizing the tribes, the early tribal governments, and the impact of the depression on the Indians—add substantially to our understanding of the Indian New Deal. Taylor concludes with a discussion of Collier's problems with Congress during the early 1940s and with a brief, preliminary assessment of the Indian New Deal.

Taylor finds that the ideal of self-determination was often subordinated to the need for fast action in what administrators perceived as the crisis situation of the depression. Much of the Indian New Deal was imposed on the Indians, and the top leadership of the Bureau failed, according to Taylor, to recognize the diversity that characterized Indian groups in the 1930s. The Indian Reorganization Act that Congress passed was considerably weaker than the act that Collier wanted, however; nor did Congress provide funds adequate to implement Collier's program. Taylor concludes that "the Indian New Deal left a mixed legacy" (p. 150). The economic development and self-determination programs fell far short of their objectives. Congressional parsimony, lingering paternalism, and a failure to understand the Indian community put limits on the extent to which the dream of self-governing, independent Indian communities could be realized. The Indian New Deal, however, helped to produce increased pride and a sense of common purpose among American Indians from different tribes.

This conclusion is similar to those reached by other historians who have been working to revise

the interpretation of the Indian New Deal presented by Collier and his associates. The narrow focus of Taylor's effort enables him to provide more detail than we have had on the implementation of the Indian New Deal on the tribal level, particularly in the Great Plains, but results in a book that is more for the specialist than the general reader.

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LESTER D. LANGLEY. *The United States and the Caribbean, 1900-1970*. Athens: University of Georgia Press. 1980. Pp. viii, 324. \$22.00.

This volume follows Lester D. Langley's earlier study that traced the United States-European struggle for supremacy in the American Mediterranean. Resuming the story at the turn of the century, the author, as in the first book, is less interested in plowing new ground than in providing a synthesis of published materials. The first work told of the struggle to become dominant; this one describes how the United States conducted affairs after achieving hegemony in this "empire without colonies." The volume contains few or no interpretive surprises. Emphasis is on diplomatic and political relations with little discussion of economic matters. The book is divided into three parts: the protectorate era ending in 1921; the period between wars when Washington went from reassessment of empire to the Good Neighbor policy; and the post-war era when the Cold War and revolutionary nationalism provided a strong challenge to United States policy.

At the beginning of the century the Caribbean provided a laboratory for Progressive ideals. There, says Langley, the United States, with mixed motives, could impose political order, economic tutelage, and civic morality. The author does not believe that Theodore Roosevelt intended to create a protectorate in the Dominican Republic when he established a customs receivership or that he intended the Roosevelt Corollary to provide a legal foundation for creating protectorates. The dollar diplomacy of President Taft and Secretary Knox brought subtle changes in Washington's policy, allowing for a more expanded role in the internal politics of Caribbean countries; the policy succeeded less by dollars than by bullets despite the hope that dollars would replace bullets. President Wilson, although professing noninterventionism, easily exceeded his two predecessors in the degree of interference in the Caribbean. During this period, Langley concludes, there were United States ac-

complishments in the Caribbean, but instilling democratic virtues and committing Caribbean governments to orderly, progressive rule were not among them.

For Langley, the importance of the interwar years in United States–Caribbean relations is in the evolution of subtle changes leading to the Good Neighbor policy, a policy marked by varied approaches to Caribbean problems if not any change in goals. In some cases, such as Cuba, the author questions how good the good neighbor really was.

Not unexpectedly, Cuba dominates the third section of the book but Guatemala, the Dominican Republic, Panama, and Puerto Rico (America's Ireland?) also receive attention. The demise of the Caribbean empire of the United States came not from the events in these nations, but, says Langley, from the historic paradox within American policy. Washington desired to advance democracy and economic progress in the Caribbean nations but at the same time violated their sovereignty, denigrated their institutions, and disparaged their culture.

There is little here that the informed reader will not recognize from other studies, but the chronicling of these events based on wide reading and the author's work in primary sources through the years provides a convenient and reliable narrative of relatively short length. The author's organization highlights special national problems in the Caribbean and changes of outlook in succeeding presidential administrations, yet the essential continuity of policy is clear. Langley is balanced in his judgments, critical but not rancorous. The book has a good annotated bibliography.

WILLIAM KAMMAN
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MELVYN P. LEFFLER. *The Elusive Quest: America's Pursuit of European Stability and French Security, 1919–1933*. Chapel Hill: University of North Carolina Press. 1979. Pp. xvi, 409. \$22.00.

"This book seeks to bridge the gap between revisionist and traditionalist studies of [post-World War I] American foreign policy," the author writes, and to an impressive degree the volume succeeds in this aim. No longer do we have to choose between the classic picture of Wilsonian internationalism/idealism giving way to Republican isolationism/naïveté (or uncertain straddle) and the later view of Americans in these years continuously driven by the need for economic expansion. In this first major postrevisionist study Melvyn P. Leffler offers us a plausible and new kind of continuity, one that emphasizes (somewhat like Graham Allison's "governmental politics model") the "complex interplay [through successive administrations] of interest-

group pressures, bureaucratic rivalries, partisan conflicts, and legislative-executive encounters" (p. 43). From this perspective what is particularly noteworthy to Leffler is the ingenuity and realism of Republican leadership as it sought in the 1920s to put together a combination of policies and decision-making instruments that would prove capable of balancing "domestic imperatives" with the requirements of European stabilization. Make no mistake, Leffler contends, Harding, Hughes, Coolidge, and Hoover considered Europe important for many reasons and were continuously involved in the struggle to reconstruct and stabilize that area. If their willingness to work within severe domestic constraints (even to the extent of relying on a banker-conducted "economic diplomacy" that by-passed Congress and the public) points up their inability to regard European prosperity and security as indispensable to the United States, theirs was after all a pre-Nazi, pre-Keynesian, pre-atomic age, and who is to say that even today we are better able to determine and balance our essential inward and outward priorities? Republican arrangements may have been too decentralized to produce a consistent impact on Europe, too nonpolitical to reassure a frightened France, and too inward-oriented to solve the difficult intergovernmental debt problem, but they also contributed much to real accomplishments such as the Washington naval treaties and the Dawes Plan.

This book is itself a significant accomplishment—a badly needed liberal synthesis—well written, thoroughly researched in primary sources (though mostly American), and certainly provocative to right and left. To be sure, I have my own qualms about it. The title could be better; indeed, it does not even make sense. Of more consequence, the volume is built too much around French-American relations, still bearing the marks of its origin as a dissertation study of this subject. There is some justification for putting France center stage, as Leffler notes, but this should not be done to the extent of obscuring the nature of (and ignoring issues raised by scholars like Michael Hogan and Frank Costigliola regarding) American relationships with Britain and with Germany. Finally, despite the fact that Leffler gives as much attention as any historian of American foreign policy in the 1920s to the comings and goings of specific statesmen and their arguments, for me there is still an important ingredient missing when he touches upon what is actually transpiring in the several countries. Too often the "constraints" on policy, both in Europe and America, are simply accepted and listed, rather than scrutinized, explained, or measured. How potent, how rigid, how deep-seated, how interrelated were they really? No assessment of interwar foreign policy can be complete without a more critical, evaluative treatment than Leffler provides of the

people, structures, and forces with which the leadership had to deal.

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THOMAS R. MADDUX. *Years of Estrangement: American Relations with the Soviet Union, 1933-1941*. Gainesville: University Presses of Florida. 1980. Pp. ix, 238. \$15.00.

This book is an analysis of Soviet-American relations between 1933 and 1941. Following an introductory chapter on the period of nonrecognition between 1917 and 1933, Thomas R. Maddux analyzes Roosevelt's decision to recognize the Soviet government; the resulting problems over debts and propaganda; the views of the American diplomats in Moscow on events in the USSR; the reaction of the American public, especially the American press, to these events; the Non-Aggression Pact the Soviet Union signed with Hitler's Germany; the Soviet invasion of Finland and the invasion of the USSR by Germany; and finally Roosevelt extending Lend-Lease to the Soviet Union.

Maddux believes that the foreign policy of the United States towards the Soviet Union between 1933 and 1941 was generally unsuccessful. He notes that few of the dreams of 1933 were fulfilled. Trade with the Soviet Union remained very modest, and diplomatic cooperation with the Soviet Union proved difficult throughout the period.

Maddux notes in some detail the opposition of the State Department to Roosevelt's policy of attempting to cooperate with the Soviet Union. Further, he comments that American public opinion worked against cooperation with the USSR, because of the unsuccessful debt negotiations and Moscow's violation of the agreements negotiated in 1933 concerning propaganda.

Another stumbling block to cordial Soviet-American relations was the view of the Soviet government, headed by Stalin. As Maddux notes, Roosevelt "never fully understood the nature of Stalin's regime" (p. 159). The president minimized "the role of ideology in Soviet policy and too quickly brushed aside fears about Soviet designs on Eastern Europe" (p. 159). Therefore, he never really understood Soviet intentions, notwithstanding the fact that a number of his advisers did. In the end, Roosevelt's efforts for cooperation with Stalin were doomed.

While Maddux notes that the first eight years of American relations with the Soviet Union did not provide any permanent benefits, recognition had brought an important observation post in the Soviet capital for the United States and contributed to the training of Soviet analysts such as George Ken-

nan and Charles Bohlen. And Roosevelt should have listened more carefully to his advisers. Maddux writes, "instead of profiting from the advice of the Soviet specialists, who advocated a strategy of firmness with quid pro quo tactics, Roosevelt favored a compromise approach and avoided difficult negotiations" (p. 161). This gave the Soviets a feeling that Roosevelt was a "somewhat weak and vacillating compromiser" (p. 161).

Maddux provides a good deal of background information that is important to understanding the diplomacy of World War II and the origins of the Cold War. His book is well researched in the American primary sources of the period and generally well written. There is much to recommend this book as a valuable contribution to Soviet-American relations, especially for those who are interested in the origins of the Cold War.

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DAVID L. PORTER. *The Seventy-Sixth Congress and World War II, 1939-1940*. Columbia: University of Missouri Press. 1979. Pp. x, 236. \$19.50.

This is an excellent scholarly study of the performance of Congress on three important foreign policy issues during the crucial years of 1939-40 when Nazi Germany was rolling over Western Europe, when Finland fought against invasion by Stalin's Soviet Union, and when Britain battled for survival against Hitler's Germany. David L. Porter of William Penn College focuses his attention on three major issues: revision of the Neutrality Act in 1939, aid to Finland, and adoption of America's first peacetime selective service law in 1940. His central thesis is that Congress "played a more significant and independent role in national decisionmaking than previously thought. Congress consistently limited the maneuverability of the president, continually placed the chief executive on the defensive, and often forced him to exhibit indecisive leadership" (p. 2). In his composite portrait of Congress he distinguishes between isolationists, moderates, and internationalists. To give greater precision to the semantics involved, Porter treats the clashes between isolationists and internationalists on the neutrality issue, between noninterventionists and interventionists on aid to Finland, and between preparedness advocates and rearmament opponents on selective service.

He refers to scholarly contributions by other historians and political scientists, but to his credit he does not use references to others as the occasion for putting them down or for tooting his own horn. For the most part, however, this little monograph is

based on intensive research in a wide range of primary sources—some not available to earlier scholars. He used manuscripts, printed sources, government records, and personal accounts. He turned to oral histories, and he interviewed some of the principals. He makes intelligent use of quantitative methods without burdening readers with jargon. Though he uses quantitative methods to examine political, ethnic, and regional patterns, he does not forget the individual in the process. He includes informative biographic sketches of key legislators from both houses. Porter's writing style is not sprightly, but both his writing and organization are clear, balanced, direct, and logical. He draws his own conclusions clearly without being dogmatic or contentious.

Porter's distinctions between isolationists and noninterventionists, between internationalists and interventionists may be more apparent than real. His references to political, ethnic, and regional variations in terms of causation or motivation may be a bit simplistic. This reviewer is a bit skeptical of Porter's conclusions on attitudes of Irish-American and German-American ethnic groups. But even if such minor reservations may be justified, Porter's analysis always provokes serious thought.

This is a fine scholarly monograph. It is pleasing to note that in a separate book Porter has also analyzed the role of the legislature on domestic issues during the Seventy-sixth Congress.

WAYNE S. COLE
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KENNETH S. CHERN. *Dilemma in China: America's Policy Debate, 1945*. Hamden, Conn.: Shoestring Press. 1980. Pp. 277. \$17.50.

Kenneth S. Chern offers an interesting partial explanation of why American policy toward China in the early postwar era diverged so much from the reality of conditions in China and of Soviet-American relations in East Asia. He demonstrates that the Truman administration, specifically its principal enforcer in the Senate, Tom Connally of Texas, succeeded in stifling what might have been a salutary public debate following the resignation of Ambassador Patrick J. Hurley in November 1945. Unfortunately, this is not the point Chern intended, for his preface promises to prove that "the vigorous China debate which actually occurred in 1945 was a critical turning point in United States China policy" (p. 10).

Chern writes thoughtfully of what might have been *if* Jack Service and John Davies had been allowed to speak, or *if* their reports had been publicized, *if* General Joseph Stilwell had come forward, *if* Senator Elbert Thomas (D-Utah) had

spoken up, *if* Connally had allowed the hearing that followed Hurley's wild charges to bring forth the substantive issues that divided Hurley from Service, Davies, and most everyone else who had observed events in China. Nonetheless, he precedes and follows his text with references to a "vigorous" or "spirited" debate. This might have been a more useful book had Chern analyzed the data he presents and dropped his original hypothesis.

Hurley's resignation focused public and congressional attention on policy toward China in late November and early December 1945. Chern looks closely at the reactions of Senators Connally, Thomas, Styles Bridges (R-New Hampshire), and Arthur Vandenberg (R-Michigan) but provides little new insight into the patterns of their thought or behavior. Quotations from "grassroots" mail in their files add color but not light. Nonetheless, Connally's role is interesting. Having concluded that much of Hurley's support in the Senate was partisan, he bullied Hurley, discrediting his testimony, without allowing careful examination of policy. Chern wishes Service, Davies, and Stilwell had testified, surmising that information they would have provided would not only have stilled Hurley but also have forced widespread awareness of the realities of Chinese politics. Hurley had drawn the spotlight to China, and the opportunity existed to educate Americans to the coming ascendancy of the Chinese Communists and to adjust Truman's policy toward accommodating that reality. Alas, it never happened.

In sum, one reading of Chern's book provides a partial answer to the question of why the United States began the year 1946 persisting in the effort to create a strong united China under Chiang Kai-shek's leadership in cooperation with the Soviet Union: because there was no debate and the administration, preoccupied by a host of issues deemed more important than China, was not forced to re-examine its assumptions or to question the policies with which it came out of the war. Connally overpowered the critics, most of whom were indifferent to China in any event, and the United States muddled on. See Dorothy Borg and Waldo Heinrich's *Uncertain Years: Chinese-American Relations, 1947-1950* (1980) for the next episode.

WARREN L. COHEN
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EVAN M. WILSON. *Decision on Palestine: How the U.S. Came to Recognize Israel*. (Hoover Institution Publication, number 218.) Stanford: Hoover Institution Press. 1979. Pp. xviii, 244. \$14.95.

Evan M. Wilson has written an informative, but by no means comprehensive, study of the United

States Palestine policy from 1942 to 1948. A retired foreign service officer, Wilson served during most of this period as Palestine desk officer in the Department of State. It is from this association that the book derives much of its strength.

With insight into both the actions and the sentiments of key department personnel, along with a fascinating behind-the-scenes account of how the department dealt with the Palestine issue, Wilson's volume is the finest yet to examine the role of the State Department in Palestine policy before the establishment of the Jewish state in 1948.

Not only does the author rely upon his position within the Near East Division of the department for information and understanding of the Palestine issue, but also his thorough review of government documents related to the department's role in making Palestine policy is commendable. Both the records of the Department of State at the National Archives, as well as recently published volumes in the Foreign Relations of the United States (FRUS) series, have been judiciously used. The documents in the FRUS series, which became available while Wilson was working on this project, give his account an important dimension that previous narratives have not matched.

Another strength of this volume is the author's clear understanding of the governmental procedures that link the State Department and the White House on policy questions. And yet it is precisely in Wilson's reliance upon recounting the details of the formal relationship between the State Department and the White House where he begins to lose his grasp on fully understanding and explaining how Palestine policy was actually shaped during the late 1940s.

Wilson continues to focus upon the activities in the Near East Division of the Department of State at a time when Palestine policy was being taken out of State and was instead being determined in the White House. There is little doubt that Wilson's own role at the Palestine desk has led him to overstate the importance of the department in determining United States policy toward Palestine.

Ironically, Wilson does recognize this changing situation in his epilogue: "As 1946 drew to a close, the Near East Division continued to be in the dark as to U.S. policy" (p. 101). Yet his earlier narrative fails to take the significance of this change into account, and he continues to focus upon the day-to-day functioning of the State Department on the Palestine issue.

A comprehensive account of the United States Palestine policy would necessitate a shift in emphasis to the White House by the end of 1946. The focus would center upon President Truman's political advisers, an examination of ethnic politics as a function of American democracy, and an analysis of

electoral strategies in presidential politics. Sadly, none of these is handled effectively in explaining who was shaping American Palestine policy and for what reasons.

Scholars of presidential politics, and ethnicity in America, will be disappointed by the superficial handling of these subjects and, even more specifically, just how the Palestine issue related to the 1948 presidential campaign. For example, with no explanation, the author asserts that the "strength of the Jewish vote, however, is apt to be exaggerated in the minds of our political leaders" (p. 153). Given the potential impact of even a small bloc of voters in a key state in the presidential electoral system, such a dismissal of America's strategically located Jewish voters in a close election is incredible.

JOHN SNETSINGER

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HAROLD F. GOSNELL. *Truman's Crises: A Political Biography of Harry S. Truman*. (Contributions in Political Science, number 33.) Westport, Conn.: Greenwood Press. 1980. Pp. xv, 656. \$35.00.

In recent years various works have been published on Truman. Revisionists have seldom found much good to say about him. More traditional historians have placed him in the near-great category. Harold F. Gosnell portrays him as a people's president. Truman was aware of the powerful American establishments, and for him the essence of the 1948 election was whether "the special privilege boys" were "going to run the country" or the people "were going to run it" (p. 372). Gosnell's strongest chapters are on that election, and he traces Truman's denunciation of such elements as the real-estate lobby that prevented passage of his housing bills and the medical lobby that fought his medicare bill. No president traveled as much or spoke with as many people as he did during that campaign. He whistle-stopped some 21,928 miles, and he made 275 speeches. The people loved it because he seemed to be fighting for them.

Gosnell helps us rediscover Truman the devoted son, husband, and father. "No affair of state... took precedence over Truman's responsibility to his mother when she needed him" (p. 11). He was similarly devoted to the other members of his family, and he was unstintingly loyal to his friends. He refused to abandon Tom Pendergast, his political godfather, even though it would have been politically expedient to do so.

In 656 pages Gosnell presents a readable, panoramic, textbook-like view of Truman's life. While he devotes too much space to such items as voting

patterns in Missouri, he does not provide sufficient analysis of important foreign policy issues, nor does he introduce much original material. Particularly distracting is his treatment of such questions as the Potsdam conference, the Wallace affair, the Marshall Plan, and Truman's recognition of Israel. We are reminded that Russia was invited to partake of the Marshall Plan, but we are not told why Stalin rejected it. He repeats the thesis that Truman recognized Israel primarily because he wanted the Jewish vote but hardly considers the impact that Russia's ambitions had on Truman, nor does he analyze the man's feelings towards the Jewish people. The diplomatic ramifications of Henry Wallace's pro-Russian leanings and of General MacArthur's military recommendations are likewise not given enough thought. At times it seems that Gossnell did not exhaust available resources and that his presentation could have been balanced better.

Nevertheless, this is a useful work, and it may inspire further research and more selective studies about the man from Independence.

HERBERT DRUKS
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MARTHA DERTHICK. *Policymaking for Social Security*. Washington: Brookings Institution. 1979. Pp. xiv, 446. Cloth \$17.95, paper \$6.95.

The social security system, embracing old-age pensions, unemployment insurance, and various forms of public assistance, was potentially the most important of all New Deal initiatives. The Social Security Administration, the agency established to administer the program, especially the pension plan, has become the most far reaching and influential of federal government domestic agencies. Yet neither the system nor the agency has received much attention from historians or, for that matter, from scholars of any discipline. According to Martha Derthick, this anomaly is the product of bureaucratic design. Believing that an expanded public sector was essential to curb the evils of a capitalist economy but that real world conditions rather than abstract theory should govern public policy, the New Deal administrators who organized the agency were tenacious but patient. Like the American Federation of Labor (and the AFL-CIO), which they made their principal ally, they sought more, always more. Expansion was their goal, accommodation their tactic. Through the years they persisted and triumphed. Disability benefits (1956) and medicare (1965) were only the most notable of their many legislative victories. To protect themselves against outsiders they created their own research organization. To allay taxpayers' concerns they spoke in platitudes and exuded sincerity. If

they could not overwhelm or "coopt" a critic, they bored him to death. Until 1972 no one cared about social security; it was successful, noncontroversial, and dull. Then the trouble began. Several long-time administrators retired, leaving the agency in the hands of less cautious expansionists. Wilbur Mills, social security's congressional godfather, developed other interests, and the AFL-CIO became less patient. In 1972, Congress enacted huge benefit and tax increases, and social security suddenly became controversial. Actuaries projected growing deficits, and taxpayers "revolted." Thus far the substantive results of the uproar have been minimal. The conspirators are still in command, their lists of legislative goals growing, presumably, ever longer. Derthick believes, nevertheless, that the time is ripe for more public discussion and an end to the conspiratorial mode of decision making that has marked the last four decades.

Derthick's account is a welcome addition to the literature of social insurance and to the "organizational synthesis" of American history. Intended as a guide to policy making, it is more valuable for what it reveals about the past. Thanks to Derthick, the American welfare state is more accessible than ever before. Thanks also to Derthick, we now have a promising framework for examining social security. Clearly it was the inexorable (in this instance) process of bureaucratic accretion rather than the clash of heroes and villains, progressives and reactionaries, or liberals and conservatives that dominated the story of social security. I can think of no other case where progressive history has so little to offer. Still, much work remains to be done. Derthick's account, regrettably, has many of the shortcomings characteristic of its genre. It is too long—a 200-page monograph expanded, like social security, into a 430-page policy analysis. The coverage is uneven; Derthick's treatment of the 1930s and 1940s is brief and superficial, while her detailed and highly technical description of recent developments will discourage all but the most persistent readers. Most serious is her neglect of the social security bureaucracy, the central element in her account. While stressing the vast influence of a handful of administrators and advisors, she provides only occasional glimpses of them, their ideas, and biases. In part, this is because they deliberately obscured their role, in part because Derthick is too concerned about policy to inquire about people. The result is an unfortunate gap. The reader learns more about the political figures, who seldom mattered, than the civil servants and lobbyists who shaped and manipulated the system.

Derthick has called attention to a subject and, even more significantly, to a political process that deserve far more extended and critical treatment than they have hitherto received. One hopes her

work will inspire others to re-examine the New Deal legacy in contemporary society and to ask, in this endeavor and others, what happens when the shouting stops.

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CANADA

PAUL-ANDRÉ LINTEAU *et al.* *Histoire du Québec contemporain: De la Confédération à la crise, 1867-1929*. Quebec: Boréal Express. 1979. Pp. 660.

Paul-André Linteau, René Durocher, and Jean-Claude Robert have embarked upon an ambitious project: a two-volume synthesis of Quebec history since 1867. This first volume is an analysis of the period ending with the onset of the Great Depression. Its successor will carry the account down to the present. That this initial volume is so admirable is a tribute both to the skill of these three young historians and to the value of a wide range of scholarly writing on recent Quebec history. A decade ago a book of this scope and quality would have been unthinkable. Whatever else the "quiet revolution" may have done to Quebec society, it certainly has had a laudable impact on historical writing.

Although this work might properly be classified as a textbook, it is not one that follows the traditional patterns. Rather it is an attempt to examine Quebec's evolution in the style of *histoire totale*. After an introduction that sets out the geographic demographic, and ethnic features of the period, the book is divided into two roughly equal parts at 1896. Each of these parts is organized around the themes of economy, society, politics, culture, and ideology. Although a certain element of dynamism is lost in this analytic approach, much else is gained. The sections on economy and society, in particular, reveal a great deal about social development and social class that would be largely ignored in the traditional narrative approach.

Several aspects of the work are striking. Anyone familiar with traditional Quebec nationalist historiography will at once be struck by the relatively small part of this study devoted to figures like Bourassa, Groulx, and other nationalist luminaries. Similarly businessmen, labor leaders, and farm spokesmen receive a fairer share of attention. Perhaps most arresting is the extent to which urban history is treated. Montreal stands at the heart of this book—perhaps not surprisingly in a book by three Montreal historians. But this results in a much better, balanced account of Quebec's social and economic growth than more politically oriented studies have offered. And it must be added

that the book treats all of Quebec, not just the French-speaking majority. The role of the English and of other ethnic groups in the province is examined in an impressively objective fashion. Although the authors appeal frequently to "class analysis," they are rarely simplistic in its application.

Naturally a book of this dimension contains a few errors of fact and some debatable interpretations. The description of Ontario's Regulation 17 concerning French language education is inaccurate (p. 560), and the contention that Ontario politicians dominated Laurier's cabinets is unproven and, I think, unprovable (p. 595). There is also some inconsistency between the assertion (p. 336) that clerico-conservative ideas have been overemphasized and the importance that is attributed to those same ideas in the chapter on "L'Eglise et l'Ecole."

In general, however, the book sets a very high standard. The authors (with some assistance from Francois-Marc Gagnon and Sylvain Simard in the cultural sections) have demonstrated a remarkable command of a wide range of information and show great skill in drawing it together. They have achieved what amounts to the first genuinely modern history of Quebec. The second volume will be awaited with impatience.

RAMSAY COOK
York University

LATIN AMERICA

PAUL E. HOFFMAN. *The Spanish Crown and the Defense of the Caribbean, 1535-1585: Precedent, Patrimonialism, and Royal Parsimony*. Baton Rouge: Louisiana State University Press. 1980. Pp. xiv, 312. \$30.00.

Paul E. Hoffman's new account of how the Castilian crown safeguarded its possessions in the Indies during the formative half-century from 1535 to 1585 divides the story into four phases. The initial period, 1535-47, set precedents for both attacks and defenses. A period of trial procedures on both sides followed, which gave way to open naval war by 1548. During the following years, international positions hardened, and the role of royal interests and obligations gained increasing importance as Spanish interests shifted from the islands to the mines of Potosí and Zacatecas. From 1564 to 1577 there emerged the well-known system of convoyed merchant fleets. The system withstood international pressures both in the emergent phase and for more years thereafter than the literature written by Spain's enemies would allow. The author maintains that the Spanish defenses were neither neglected nor ineffective.

This book is intricately structured. Its narrative is

chronological and preserves a sense of place, while the interpretation of events described considers many elements. The author enumerates those factors that he isolates as explaining the evolution of Spanish defenses in the Caribbean: (1) strategic geography; (2) the evolution of military technology; (3) diplomatic and other events in Europe; (4) the concept and politics of the king's obligation to defend his subjects and their interests; (5) the changing state of the crown's fiscal affairs; (6) the population resources of the Spanish Caribbean; and (7) the availability of war materiel and the changing pattern of corsair activity (pp. 4-5). Hoffman considers points four and five as documenting the patrimonialism and parsimony of the crown, which, together with precedent, shaped the policy of Spain.

While describing Spanish defenses, Hoffman also pays attention to surrounding circumstances. On the international level, he examines the blending of war and peace, the lack of discrimination between acts of aggression and attempts to trade, and the interpretation of ambiguous treaties by diplomats. On the national scale, attention is drawn to the conflict between the assumption of the crown's obligations to pay for the safety of its subjects and the expectation that responsibility for defense of the colonists and merchants should rest with them.

The backbone of the story is made up of the records of nineteen Spanish treasuries from around the Caribbean and from the Casa de la Contratación. These are augmented by an impressive body of auxiliary documentation from many different sources. Complexities are described in a sober style and the repetition of the major points marshals the diverse sources as the argument proceeds.

Ample statistical data put the thesis upon a firm documentary base, and they are effectively and modestly presented. The index is rather summary, but the notes are ample, precise, and helpful. With patience and skill, Hoffman redresses many hasty generalizations and answers the repeated request by scholars to know the story of Spanish defenses as seen from the Spanish perspective.

URSULA LAMB
University of Arizona

CLAUDIO VÉLIZ. *The Centralist Tradition of Latin America*. Princeton: Princeton University Press. 1980. Pp. xii, 355. Cloth \$22.50, paper \$9.75.

This important work is a lucid, vigorously argued, and comprehensive cultural interpretation of Latin American political history, designed to explain the near-universal pattern of present-day authoritarian government. The elements of the argument were present in two influential essays of the late sixties,

which still retain much of their validity. The book is filled with fresh and intriguing insights, particularly on the relation between culture and politics.

Claudio Véliz contends that Latin America fell heir to political centralism, originating in the consolidation of the Castilian state under Ferdinand and Isabella and imposed on the New World, unhindered by the restraints of feudalism. However, the critical era for Véliz was the eighteenth century, that of Bourbon "recentralization," paralleled in Brazil by the policies of the Marquis de Pombal. The major divergence from centralism was the "Liberal Pause" (1830-1930) when the Latin American elites rejected their Iberian traditions and turned outward, imitating French and British taste and style, experimenting with federalism, constitutions, and laissez faire. The centralist pattern re-emerged in the Great Depression. With the demise of export-based affluence, the state became the impetus for industrialization, the patron of labor movements and social welfare, and the source of privilege for a broad clientele of political interest groups. Because Latin America has now returned to its "centralist mainstream," Véliz foresees few structural changes, except that the current rash of military regimes may give way to more traditional civilian authoritarianism. Véliz goes beyond political chronology. He argues that political centralism was upheld by sophisticated pre-industrial cities, home of bureaucrats, miners, and hacendados long before the appearance of industrial classes that might have posed a challenge to the state. Véliz also emphasizes "latitudinarian religious centralism" in Latin America, the absence of religious nonconformity, and the resulting political resistance to central state authority.

Because the book is wide ranging and because the author's use of authorities is eclectic and sometimes whimsical, historians will have many criticisms, depending on their specialties. Moreover, the book lacks the sophistication (and also the jargon) of many recent studies of bureaucratic authoritarianism and structural dependency by political sociologists.

For me, the major inadequacy of the work (and I admit to having long been a Véliz fan) is its one-dimensional and distorted treatment of Latin America's liberal century. Perhaps because of his education and long residence in England and Australia, the author's view of political liberalism is decidedly Anglo-Saxon. Its essence is manifest in constitutionalism, laissez faire, and limitation of central authority, derived from feudalism and religious nonconformity. He ignores secularization, perhaps the major feature of liberalism in Catholic Europe. The creation of the secular state, derived from the eighteenth-century Bourbons, the French Jacobins, and the Cadiz liberals, was a major goal of Latin Ameri-

cans, not only in Mexico, but also in the Southern Cone. Recognizing anticlericalism as liberal only serves to strengthen Véliz's general argument, because the central state, far from merely surviving in the nineteenth century, was actually strengthened by this vital aspect of liberal ideology. Thus Latin American liberals were not quite the egregious imitators of exotic ideas that Véliz makes them out to be. They turned principally to Spain and to France because the problems of these countries were analogous to their own. They also turned to Jeremy Bentham, the most "continental" and statist of British liberals.

Véliz has a jaundiced view of Latin American intellectuals and of the role of ideas in politics. Thus it is ironic that his book has the quality of an essay of ideas, in the best tradition of the classic Latin American *pensadores*.

CHARLES A. HALE
University of Iowa

SERGIO VILLALOBOS R. *La economía de un desierto: Tarapacá durante la Colonia*. Santiago, Chile: Ediciones Nueva Universidad. 1979. Pp. 278.

Sergio Villalobos feels that one of the reasons for the significance of his work lies in the fact that the study of the economic history of Tarapacá is the study in miniature of the economic and social system of the Spanish empire in the colonial period. That system was characterized by commercial subjugation, vigilance of the state over mineral production, the export of precious metals, the sale of Castilian merchandise, the use of the *encomienda* and *mita* for Indian labor, the despoilment of the land and water, the emergence of wealthy families, the imposition of harsh labor upon the peons and slaves, and a legacy of misery. The author states that describing these developments as they occurred in a desert area gives his account a new and special aspect. The text bears him out.

The central theme of the work is that the fate of the natives was to work for others who were intermediaries of a more advanced economy that was greedy for gold and silver and demanded even the sweat of the Indians of a forgotten desert. The riches of the region went to the large cities and misery remained in the desert.

The author has written an excellent description of the economic, geographic, and governmental system of Tarapacá during the colonial period. He devotes considerable attention to the development of the marine, agricultural, and mineral resources of the province. Special attention is given to the labor system, including the *encomienda* and *mita* as well as free and slave labor. Because sources for some areas of the economic history are inadequate or

scarce, not all aspects of the account can be treated in depth. However, the author has found sufficient materials to enable him to accurately determine the general development. The work is well documented and has an extensive bibliography. There is also a list that includes terms related to agriculture and mining, defined as they were used in Peru and Chile in the eighteenth century, and words with a special meaning in Tarapacá.

Villalobos feels that he has written a general economic history of the region that will be augmented by later studies. He urges further research utilizing sources in the archives of Peru, Bolivia, and Spain. The author has written an interesting work that contributes significantly to our understanding of the colonial economy of Tarapacá and, by extension, of the economic and social system of the Spanish empire during the colonial era.

ROBERT D. TALBOTT
University of Northern Iowa

BRIDGET BRERETON. *Race Relations in Colonial Trinidad, 1870-1900*. New York: Cambridge University Press. Pp. x, 251. \$34.50.

The period 1870-1900 was not a truly formative one in the history of Trinidad, nor did it contain any particularly momentous events. Whereas the preceding thirty years, covered in Donald Wood's *Trinidad in Transition* (1968), had seen the immediate adjustments of a society to the abolition of slavery and the introduction of new racial elements on a large scale, the last three decades of the nineteenth century saw the entry of only the Syrio-Lebanese. Thus Brereton's *Race Relations in Colonial Trinidad* presents a somewhat static picture. But the cosmopolitan character of Trinidadian society means that it is a fascinating picture, and Brereton fills it out in excellent detail.

It is Brereton's thesis that "the upper class of the society controlled and determined cultural and social life" (p. 57) and that the Afro-Creole masses possessed a submerged culture that coexisted alongside that of the white elite. Although the white elite had its own internal divisions, particularly between the British colonial officials and the Anglo and French Creoles (the latter clinging to a supposed aristocratic heritage dating from the eighteenth century), there is no doubt that the whites formed an essentially unitary and dominant class. Between the white elite and the Afro-Creole masses fell the colored and black middle class, and it is the emergence of this group that constituted one of the most obvious social changes of the period. This bourgeoisie depended for its status not on wealth or political power, but on education, which permitted its members to aspire to employment in teaching and

the civil service. Few black Trinidadians were successful in attaining this middle-class status, and, of those who did, many suffered an ambivalence to those above and below them in the class structure.

The other major racial group in the society was the Indians, who made up one-third of the total population of Trinidad by 1900. Yet they remained relatively isolated, most of them living on sugar plantations, and Brereton argues that they "were regarded as an exotic group, marginal to Trinidad society, insufficiently integrated to be considered a part of it" (p. 177). For this reason the Indians play a small role in Brereton's study and disappear almost entirely in her concluding discussion of racism and race relations.

Although Brereton makes little use of the tools of the "new social history," she does provide us with a vivid feeling for the nature of life in the houses of the elite and the barrack-yards of the masses. The discussion of carnival and its social function is particularly interesting, since this period was one of crisis for that fête. Brereton's book is crucial to any understanding of the development of Trinidadian society.

B. W. HIGMAN
University of the West Indies

JUDITH LAIKIN ELKIN. *Jews of the Latin American Republics*. Chapel Hill: University of North Carolina Press. 1980. Pp. xv, 298. \$17.00.

Between 1840 and 1942, about 376,000 European Jews, or slightly less than 10 percent of the total Jewish emigration during this period, entered Latin America. These immigrants formed important Jewish communities, especially in Argentina, which ranked third in the world as a country of Jewish immigration, but also in Brazil, Chile, Uruguay, and Mexico. Despite the importance of this migration, no satisfactory scholarly study of Latin American Jews existed until Judith Laikin Elkin published this book.

The author tells us that her purpose is "to stake out the relatively new field of Latin American Jewish studies" (p. xiv). Her book, which is thoroughly researched (Elkin uses Yiddish and Hebrew sources) and well written, accomplishes that task admirably, but it does much more. Elkin carefully analyzes the patterns of Jewish migration to Latin America, the impact this migration made on the region's social and economic structures, and the complex relationships between Jews and Latin American culture. In brief, she has made a major contribution to Latin American immigration and social history.

The book's scope is impressive. Elkin begins with a survey of Jewish life in the Iberian Peninsula and during the Latin American colonial period. She

continues with a fascinating chapter that examines nineteenth-century Jewish migration (to 1889) in the context of the immigration policies the new republics were formulating. The bulk of the book, however, is devoted to a thorough analysis of the massive Jewish immigration that began in 1889 and continued, with some interruptions, until World War II. Here she examines in detail the impact Ashkenazim Jews (who formed the bulk of Jewish migrants to Latin America) made on the development of commerce and industry. In this and other sections Elkin examines the Jewish impact in the smaller as well as the larger countries. Elkin also presents the most balanced and objective treatment of the famous Argentine Jewish agricultural colonies that this reviewer has seen.

There is a wealth of fascinating information in the chapter on "Community Life." The chapter "Demography of Latin American Jewry" is particularly important. Here Elkin demonstrates that previous estimates of Jewish population were inflated, and in fact the Jewish population in all republics except Ecuador and Venezuela is dwindling in size. The final chapters examine the socioeconomic status of the Latin American Jewish communities today and compare the Jewish experience in North and South America.

One point of criticism involves Elkin's sometimes questionable theoretical context. She relies on a traditional cultural determinist approach when emphasizing that Latin American elites neglected entrepreneurship in the commercial and industrial fields that Jews so frequently entered. But, as the research of William J. Fleming and Roberto Cortés Conde has shown, the Argentine elites at the turn of the century included many highly effective entrepreneurs. Elkin might also have analyzed the roots and nature of Latin American antisemitism more systematically.

CARLE E. SOLBERG
University of Washington

JOHN HOYT WILLIAMS. *The Rise and Fall of the Paraguayan Republic, 1800-1870*. (Latin American Monographs, number 48.) Austin: Institute of Latin American Studies, University of Texas at Austin. 1979. Pp. x, 286. Cloth \$17.95, paper \$6.95.

With the collapse of Spanish authority in the Rio de la Plata after 1810, Paraguay alone of that vice-royalty's provinces was spared the turmoil of the next two generations, largely because of the policies of the first two dictators, José Gaspar de Francia (1814-40) and Carlos Antonio López (1841-62), who manipulated a deeply felt sense of Paraguayan nationality to control and protect the nation. The tranquility and relative prosperity of these years

evanesced after 1862, as Francisco Solano López welcomed participation in a tragic war. How and why this came about is the subject of John Hoyt Williams's fine study.

Developing arguments advanced in earlier articles, Williams shows that the "hermit state" of Paraguay sprang not from conscious exoticism but was merely the best alternative available to Dr. Francia. Williams analyzes El Supremo's faults and records his excesses but concludes that Francia's "populist" dictatorship allowed most Paraguayans to live better lives than their ancestors or contemporaries elsewhere in the Plata region.

Positive accomplishments of the regime of the elder López in the 1840s and especially the 1850s resulted from the continuation of his predecessor's policies of neutrality and noninterference in regional quarrels and maintenance of order internally. Carlos Antonio was thus able to oversee a limited but significant modernization and safeguard his nation's security. Williams shows that this developmental program arose not from the Corpulent Despot's alleged liberalism; instead, he hoped to strengthen his nation against its covetous neighbors. Paraguayan material progress was financed atypically from current resources, without foreign loans, investment, or control.

Benefits of modernization were then smashed in

Paraguay's suicidal struggle with the Triple Alliance. Williams properly dismisses apologies for López heard in Paraguay and elsewhere and accurately assesses Francisco Solano's culpability in foolishly initiating and criminally prolonging the war, as he "left reality far behind him" (p. 221).

Williams's approach is traditional, concentrating on politics and administration, commerce, diplomacy, and military matters. His able execution, however, makes the book supersede previous works. Its merits are extensive archival research, judicious use of published materials, a sound view of Paraguayan culture, and a colorful narrative. Not all readers will accept Williams's conclusions unconditionally. Some including the reviewer, will prefer to identify Francia's system as state capitalism rather than state socialism. Others might disagree with such things as the author's interpretation of crime statistics for Afro-Paraguayans, his bold, often humorous depiction of people and events, or his interpretation of racial nationalism. Those who protest his findings, though, must be prepared to confront his extensive evidence. Williams has now replaced Julio César Chaves as the standard authority for the period.

JAMES SCHOFIELD SAEGER
Lehigh University

Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

PEREZ ZAGORIN, editor. *Culture and Politics from Puritanism to the Enlightenment*. (Publications from the Clark Library Professorship, UCLA, number 5.) Berkeley and Los Angeles: University of California Press, for the William Andrews Clark Memorial Library. 1980. Pp. xiii, 284. \$14.50.

J. H. HEXTER, Property, Monopoly and Shakespeare's *Richard II*. CHARLES GRAY, Reason, Authority, and Imagination: The Jurisprudence of Sir Edward Coke. RICHARD H. POPKIN, Jewish Messianism and Christian Millenarianism. J. G. A. Pocock, Post-Puritan England and the Problem of the Enlightenment. JOHN M. WALLACE, John Dryden's Plays and the Conception of an Heroic Society. RICHARD S. WESTFALL, Isaac Newton in Cambridge: The Restoration University and Scientific Creativity. ROBERT M. ADAMS, In Search of Baron Somers. ISAAC KRAMNICK, Children's Literature and Bourgeois Ideology: Observations on Culture and Industrial Capitalism in the Later Eighteenth Century. RONALD PAULSON, Burke's Sublime and the Representation of Revolution.

J. CLARKE *et al.*, editors. *Working-Class Culture: Studies in History and Theory*. New York: St. Martin's Press. 1979. Pp. 301. \$19.95.

CHAS CRITCHER, Sociology, Cultural Studies and the Post-war Working Class. RICHARD JOHNSON, Culture and Historians. RICHARD JOHNSON, "Really Useful Knowledge": Radical Education and Working-Class Culture, 1790–1848. MICHAEL BLANCH, Imperialism, Nationalism and Organized Youth. PAM TAYLOR, Daughters and Mothers—Maids and Mistresses: Domestic Service between the Wars. PAUL WILD, Recreation in Rochdale, 1900–40. CHAS CRITCHER, Football since the War. PAUL WILLIS, Shop-Floor Culture, Masculinity and the Wage Form. RICHARD JOHNSON, Three Problematics: Elements of a Theory of

Working-Class Culture. JOHN CLARKE, Capital and Culture: The Post-war Working Class Revisited.

NORMAN HILLMER and PHILIP WIGLEY, editors. *The First British Commonwealth: Essays in Honour of Nicholas Mansergh*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1980. Pp. 192. \$25.00.

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SERGIO BERTELLI *et al.*, editors. *Florence and Venice: Comparisons and Relations*. Volume 1, *Quattrocento*. (Harvard University Center for Italian Renaissance Studies, Villa i Tatti, number 5.) Florence: Nuova Italia Editrice. 1979. Pp. vi, 252. L. 10,000.

GENE BRUCKER, Humanism, Politics and the Social Order in Early Renaissance Florence. FELIX GILBERT, Humanism in Venice. MYRON P. GILMORE, *Studia Humanitatis* and the Professions in Fifteenth Century Florence. FRANCESCO TATEO, Marcantonio Sabellico e la Svolta del Classicismo Quattrocentesco. CESARE VASOLI, Comments. GIORGIO CRACCO, Patriziato e Oligarchia a Venezia nel Tre-Quattrocento. NICOLAI RUBINSTEIN, Oligarchy and Democracy in Fifteenth-Century Florence. ROSLYN L. PESMAN, Comments. SERGIO BERTELLI, La Politica Estera Fiorentina e Quella Veneziana nella Crisi Rinascimentale. MICHAEL

MALLET, Preparations for War in Florence and Venice in the Second Half of the Fifteenth Century. ANGELO VENTURA, Il Dominio de Venezia nel Quattrocento. ISABELLE HYMAN, The Venice Connection: Questions about Brunelleschi and the East. CRAIG HUGH SMYTH, Venice and the Emergence of the High Renaissance in Florence: Observations and Questions. JAMES BECK, Comments.

PETER LANGE and SIDNEY TARROW, editors. *Italy in Transition: Conflict and Consensus*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1980. Pp. 186. \$25.00.

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DIMITRIJE DJORDJEVIC, editor. *The Creation of Yugoslavia, 1914-1918*. Santa Barbara, Calif.: Clio Brooks, with the Center for Russian and East European Studies, University of California, Los Angeles. 1980. Pp. viii, 228. \$19.50.

DIMITRIJE DJORDJEVIC, The Idea of Yugoslav Unity in the Nineteenth Century. MILORAD EKMEČIĆ, Serbian War Aims. ALEX N. DRAGNICH, The Serbian Government, the Army, and the Unification of Yugoslavs. GALE STOKES, The Role of the Yugoslav Committee in the Formation of Yugoslavia. MICHAEL B. PETROVICH, Russia's Role in the Creation of the Yugoslav State, 1914-1918. DOMNA VISVIZI DONTAS, Troubled Friendship: Greco-Serbian Relations, 1914-1918. FRITZ FELLNER, George D. Herron and the Italian-Yugoslav Rivalries during the Final Stages of World War I, 1917-1919: Some Observations Based on Documents from the Herron Papers. JOHN R. LAMPE, Unifying the Yugoslav Economy, 1918-1921: Misery and Early Misunderstandings. HENRIK BIRNBAUM, Language, Ethnicity, and Nationalism: On the Linguistic Foundations of a Unified Yugoslavia. WAYNE VUCINICH, The Formation of Yugoslavia. ANDREJ MITROVIĆ, The 1919-1920 Peace Conference in Paris and the Yugoslav State: An Historical Evaluation.

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ALEXANDER RABINOWITCH, Introduction. STEPHEN F. COHEN, The Friends and Foes of Change: Reformism and

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WILLIAM W. CUTLER III and HOWARD GILLETTE, JR., editors. *The Divided Metropolis: Social and Spatial Dimensions of Philadelphia, 1800-1975*. (Contributions in American History, number 85.) Westport, Conn.: Greenwood Press. 1980. Pp. xviii, 308. \$25.00.

HOWARD GILLETTE, JR., The Emergence of the Modern Metropolis: Philadelphia in the Age of Its Consolidation. JEFFREY P. ROBERTS, Railroads and the Downtown: Philadelphia, 1830-1900. DEBORAH C. ANDREWS, Bank Buildings in Nineteenth-Century Philadelphia. GEORGE E. THOMAS, Architectural Patronage and Social Stratification in Philadelphia between 1840 and 1920. DENNIS CLARK, "Ramcat" and Rittenhouse Square: Related Communities. STEPHANIE W. GREENBERG, The Relationship between Work and Residence in an Industrializing City: Philadelphia, 1880.

MARGARET S. MARSH, The Impact of the Market Street "El" on Northern West Philadelphia: Environmental Change and Social Transformation, 1900-1930. MEREDITH SAVERY, Instability and Uniformity: Residential Patterns in Two Philadelphia Neighborhoods, 1880-1970. JOHN F. BAUMAN, Public Housing in the Depression: Slum Reform in Philadelphia Neighborhoods in the 1930s. WILLIAM W. CUTLER III, The Persistent Dualism: Centralization and Decentralization in Philadelphia, 1854-1975.

D. A. BRADING, editor. *Caudillo and Peasant in the Mexican Revolution*. (Cambridge Latin American Studies, number 38.) New York: Cambridge University Press, 1980. Pp. x, 311.

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urnino Cedillo: A Traditional Caudillo in San Luis Potosí, 1890-1938. HEATHER FOWLER SALAMINI, Revolutionary Caudillos in the 1920s: Francisco Múgica and Adalberto Tejeda. GILBERT M. JOSEPH, Caciquismo and the Revolution: Carrillo Puerto in Yucatán. RAYMOND BUVE, State Governors and Peasant Mobilisation in Tlaxcala. HANS WERNER TOBLER, Conclusion: Peasant Mobilisation and the Revolution.

WOLFSKILL, GEORGE and DOUGLAS W. RICHMOND, editors. *Essays on the Mexican Revolution: Revisionist Views of the Leaders*. (The Walter Prescott Webb Memorial Lectures, number 13.) Austin: University of Texas Press. 1979. Pp. xxi, 136. \$9.95.

GEORGE WOLFSKILL and DOUGLAS W. RICHMOND, Preface. MICHAEL C. MEYER, Introduction. WILLIAM H. BEEZLEY, Madero: The "Unknown" President and His Political Failure to Organize Rural Mexico. FRIEDRICH KATZ, Villa: Reform Governor of Chihuahua. DOUGLAS W. RICHMOND, Carranza: The Authoritarian Populist as Nationalist President. DAVID C. BAILEY, Obregón: Mexico's Accommodating President. LYLE C. BROWN, Cárdenas: Creating a *Campesino* Power Base for Presidential Policy.

Documents and Bibliographies

The following collections of documents, bibliographies, and other similar works were received by the *AHR* between June 13, 1980 and August 20, 1980. Books that will be reviewed are not usually listed, but listing does not necessarily preclude subsequent review.

GENERAL

- ADLER, ALAN, editor. *Theses, Resolutions, and Manifestoes of the First Four Congresses of the Third International*. Introduction by BERTIL HESSEL. Translated by ALIX HOLT and BARBARA HOLLAND. London: Ink Links or Humanities Press, Atlantic Highlands, N.J. 1980. Pp. xxxiv, 481. \$33.00.
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- SCOTT, A. F. *The Saxon Age: Commentaries of an Era*. London: Croom Helm; distributed by Biblio Distribution Center, Totowa, N.J. 1979. Pp. 182. \$20.00.

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- MORRILL, J. S. *Seventeenth-Century Britain, 1603-1714*. (Critical Bibliographies in Modern History.) Folkestone, Kent: William Dawson and Sons or Archon Books, Hamden, Conn. 1980. Pp. 189. \$17.50.
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- BREEDEN, JAMES O., editor. *Advice Among Masters: The Ideal in Slave Management in the Old South*. (Contributions in Afro-American and African Studies, number 51.) Westport, Conn.: Greenwood Press. 1980. Pp. xxvi, 350. \$25.00.
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- RADFORD, ARTHUR W. *From Pearl Harbor to Vietnam: The Memoirs of Admiral Arthur W. Radford*. Edited by STEPHEN JURIKA, JR. (Hoover Institution Publication, number 221.) Stanford: Hoover Institution Press. 1980. Pp. xii, 476. \$15.00.
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- GREENWOOD, F. MURRAY, editor and translator. *Land of a Thousand Sorrows: The Australian Prison Journal, 1840-1842, of the Exiled Canadian Patriote, François-Maurice Lepailleur*. Vancouver: University of British Columbia Press. 1980. Pp. xxxvi, 174. \$25.00.
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Other Books Received

Books listed were received by the AHR between June 13, 1980 and August 20, 1980. Books that will be reviewed are not usually listed, but listing does not necessarily preclude subsequent review.

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- ABEL, WILHELM. *Agricultural Fluctuations in Europe: From the Thirteenth to the Twentieth Centuries*. Translated by OLIVE ORDISH. Foreword by JOAN THIRSK. New York: St. Martin's Press. 1980. Pp. xii, 363. \$40.00.
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- BERGESEN, ALBERT, editor. *Studies of the Modern World-System*. (Studies in Social Discontinuity.) New York: Academic Press. 1980. Pp. xiv, 281. \$25.00.
- BILSKY, LESTER J., editor. *Historical Ecology: Essays on Environment and Social Change*. (National University Publications.) Port Washington, N.Y.: Kennikat Press. 1980. Pp. 195. \$13.50.
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- DAHRENDORF, RALF. *Life Chances: Approaches to Social and Political Theory*. Chicago: University of Chicago Press. 1979. Pp. ix, 181. \$15.00.
- DRAY, WILLIAM. *Perspectives on History*. Boston: Routledge and Kegan Paul. 1980. Pp. ix, 142. Cloth \$20.00, paper \$10.00.
- ENGELHARDT, DIETRICH VON. *Historisches Bewusstsein in der Naturwissenschaft von der Aufklärung bis zum Positivismus*. (Orbis Academicus: Problemgeschichten der Wissenschaft in Dokumenten und Darstellungen, Sonderband, number 4.) Freiburg: Verlag Karl Alber. 1979. Pp. 260.
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- JOYCE, CHARLES PATRICK. *Sarajevo Shots*. (Studies in Revisionist Historiography.) New York: Revisionist Press. 1979. Pp. 297.
- KLEIN, FRITZ, editor. *Neue Studien zum Imperialismus vor 1914*. (Akademie der Wissenschaften der DDR, Schriften des Zentralinstituts für Geschichte, number 63.) Berlin: Akademie-Verlag. 1980. Pp. 241. 24 M.
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- LARRAIN, JORGE. *The Concept of Ideology*. Athens: University of Georgia Press. 1979. Pp. 256. \$18.00.
- LINDBLOM, CHARLES E. *Politics and Markets: The World's Political-Economic Systems*. Reprint. New York: Basic Books. 1980. Pp. xi, 403. Paper \$5.95.
- MERCHANT, CAROLYN. *The Death of Nature: Women, Ecology, and the Scientific Revolution*. San Francisco: Harper and Row. 1980. Pp. xx, 348. \$16.95.
- MILWARD, ALAN S. *War, Economy, and Society, 1939-1945*. (History of the World Economy in the Twentieth Century, number 5.) Reprint. Berkeley and Los Angeles: University of California Press. 1979. Pp. xiii, 395. Paper \$6.95.
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- ROSTOW, W. W. *Why the Poor Get Richer and the Rich Slow Down: Essays in the Marshallian Long Period*. Austin: University of Texas Press. 1980. Pp. xvii, 376. \$19.95.
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- ASHLEY, MAURICE. *England in the Seventeenth Century*. Rev. ed. New York: Barnes and Noble. 1980. Pp. 268. Cloth \$20.00, paper \$9.95.
- HESKIN, KEN. *Northern Ireland: A Psychological Analysis*. New York: Columbia University Press. 1980. Pp. xii, 174. \$17.50.
- PRINGLE, ROGER, editor. *A Portrait of Elizabeth I in the Words of the Queen and Her Contemporaries*. Totowa, N.J.: Barnes and Noble. 1980. Pp. 128. \$10.75.
- ROTHSTEIN, ANDREW. *The Soldiers' Strikes of 1919*. London: Macmillan; distributed by Humanities Press, Atlantic Highlands, N.J. 1980. Pp. viii, 114. \$27.00.
- WHYTE, J. H. *Church and State in Modern Ireland, 1923-1979*. 2d ed. Totowa, N.J.: Barnes and Noble or Gill and Macmillan, Dublin. 1980. Pp. xiv, 491. \$32.50.
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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

TO THE EDITOR:

Congratulations to Donald Roden for this fine article, "Baseball and the Quest for National Dignity in Meiji Japan" (*AHR*, 85 [1980]: 511-34). There seems to me, however, to be an omission of some importance—namely, the matter of umpires. How were the umpires chosen for those games? Were they accused of partiality by one side or the other? Perhaps Professor Roden could provide an additional paragraph or so on that topic.

BERNARD SINSHEIMER
University of Maryland,
European Division,
Boulogne-Billancourt, France

PROFESSOR RODEN REPLIES:

My thanks to Professor Bernard Sinsheimer. The selection and performance of the umpires did not become an issue during the Ichikō-Yokohama series. Anxious for just the opportunity to play the Yokohama Athletic Club, the Ichikō students were resigned to having resident Americans serve as umpires for the games. Moreover, both sides accepted the latest *Spalding Guide* on rules of play; and, since most of the games were so lopsided anyway, there were apparently no disputes over "close calls." The umpiring did, however, spark controversy in subsequent inter-Pacific matches that took place in Japan and were supervised by Japanese officials. Spokesmen for the University of Wisconsin baseball team, for example, attributed three of their losses to Keio University in 1909 to "the unfairness of the native umpires" (1911 *Badger*, 192).

DONALD RODEN
Rutgers University

TO THE EDITOR:

In writing our reply to the comments of Stanley L. Engerman and Thomas B. Alexander in the December *Review* ("Antebellum North and South in Comparative Perspective: A Discussion," *AHR*, 85 [1980]: 1150-66), we had also hoped on that occasion to offer our comments on Edward Pessen's article, "How Different from Each Other Were the Antebellum North and South?" (*ibid.*, 1119-49). Pessen objected to that procedure, however, and we are therefore resorting to this separate letter.

What bothers us about Professor Pessen's article is its methodological assumptions: the testing method bears no relation to the question asked. Pessen's testing is based upon an analysis of the social and political structure of the antebellum North and South, mainly as reflected by census material on the distribution of property holdings, by voting requirements, and by data on the socioeconomic status of elected officials. He found that, in both sections, the rich were relatively few and owned an outsized share of all property; the poor were more numerous and owned far less property. It was unusual for the poor to become rich. Most of the free adult males among the common folk in both sections could vote, but they elected mainly people from the upper economic strata, and so politics effected few changes in the social order.

Let us consider, first, the matter of similarities between Northern and Southern social structure (as Professor Pessen measured them, by statistics on the relative distribution of property). The flaw in the method is its irrelevance to the question: it can be used to "prove" that obviously different societies were pretty much the same. By using the distribution of property as the index, one can show that the South in 1880 had not changed since the war or that the American socioeconomic pyramid in 1930, as reported by Means and Berle, was about the same as it had been in 1830. It probably still is. It was in New Zealand in 1890. Historically, it doubtless has been in Europe. Indeed, if due allowance is made for the fact that the counters in the status game are different in noncommercial cultures, a

similar social structure probably pertained among Charlemagne's Franks and Shaka's Zulus. All that Pessen's data prove is that, when status is measured in wealth, a few rich people are at the top and a lot of poor people are at the bottom. This says nothing about the similarities or differences between Northern and Southern cultures or between any other cultures.

But Professor Pessen's data are not merely irrelevant, they are also misleading, for they are based upon an ahistorical and culture-bound conception of property. Property is a set of rights to the use and usufruct of things, including animals and (in some societies, among them the antebellum South) the labor of others. These rights vary with time and place and provide revealing clues to the nature and values of a society. In the Old South, the most important single property right, for the vast majority of white inhabitants, was that of grazing animals on all unfenced land, no matter who owned the rights to other uses of the land. The value of this right cannot be measured in numbers. Property rights in the North, by contrast, can be expressed mainly in numbers, for there private property rights in land were more nearly absolute. Thus, there is a *qualitative* difference in the forms of property and wealth in the two cultures—which means, logically, that the two cannot be measured by the same criteria. That, ipso facto, proves that they were fundamentally different.

As for the political part of Professor Pessen's testing procedure, even if planters did hold most of the offices, what counts is not who holds office but what one can do with it. As we said in the December issue, the inability of Southern planters to obtain general fencing laws is a case in point, and so is the general Southern pattern of taxing the property of the planters and exempting from taxation the principal property of the plain folk. If one has power, one does not saddle one's self with the tax burden and exempt the powerless.

We offer these correctives only because we believe that Professor Pessen's article is a prime example of a practice that has unfortunately become quite widespread in our profession—namely, the use of quantitative data in a way that belies common sense and the nearly universal observations of contemporaries. We submit a single figure that, we believe, attests to the irrelevance of all of Pessen's numbers purporting to show similarities between North and South. The figure is 600,000—the number of Civil War graves.

FORREST MCDONALD
GRADY MCWHINEY
*Center for the Study of Southern
History and Culture,
University of Alabama*

PROFESSOR PESSEN REPLIES:

Forrest McDonald and Grady McWhiney write that I "objected to [the] procedure" whereby they had hoped "to offer their comments" on my article as part of their reply to their *AHR Forum* critics. Since they are aware of the facts, Professors McDonald and McWhiney no doubt meant to put the matter accurately, but they have not altogether succeeded in doing so.

On February 19, 1980, Otto Pflanze, the editor of the *American Historical Review*, advised me that the *AHR* wished to publish my essay in the *Forum*, "with responses by two historians . . . and a reply by [me] in accordance with [the *AHR*'s] usual format." I replied, expressing my delight. On April 2 Pflanze wrote to say that he now wished Professors McDonald and McWhiney and me to "react [in print] to each other's theses as well as to the comments by [Thomas B.] Alexander and [Stanley L.] Engerman." "What we seek," he said, "is a lively but unpolemical debate." As soon as I received this letter, I dashed off a brief response to it, which I communicated to Pflanze by telephone on April 8. I assured him that, while I, flexible soul that I am, was ready to work within any format, including the one he had suggested, the new format struck me as unrewarding. "It partakes," I said, "of the bloodletting of a Roman circus and does so for no good reason that I can see. There are limits, after all, within which intellectual excitement should be sought." I added that, "from what I have previously read of the McD and McWh thesis, I of course do not agree with it. But why on earth should I be asked formally to publicize my criticism when I have no wish to do so?" Pflanze told me immediately that he saw my point, agreed with it, and would notify McDonald and McWhiney that the "usual *AHR Forum* format" would be followed and that "there will be no 'tag team wrestling' in the December issue." It is clear the Pflanze did notify them, and I thank him for doing so.

Much ado about little, I agree, but I do like to set the record straight.

Now let me respond to the "corrective" of my essay offered by Professors McDonald and McWhiney. Their summary of my argument is unavoidably brief—but unnecessarily distorted. The point is not that the rich "owned an outsized share"; they always do. What is remarkable is how *similar* was this share in both the North and the South. And the point is not simply that people from "the upper economic strata" were usually elected. What is more interesting is that such people were usually *offered* by the major parties.

Professors McDonald and McWhiney find irrelevant to an understanding of Northern and South-

ern cultures my comparison of sectional distributions of wealth. Here they appear to rely on a most narrow definition of culture. McDonald, particularly, surprises me, for only two years ago, in presenting a composite portrait of Celtic culture as described by observers from Julius Caesar to Frank L. Owsley, he wrote, "Their society is hierarchical. A small . . . upper class has by far the greatest share of the people's wealth . . . Everyone else is, in varying degrees, dependent on them" ("The Ethnic Factor in Alabama History: A Neglected Dimension," *Alabama Review*, 31 [1978]: 257). Almost 150 years ago Tocqueville observed that the distribution of wealth that he thought obtained in the United States was "the fundamental fact from which all others seem to be derived." The work of James Henretta, Robert Doherty, Robert E. Gallman, and many other historians has shown that it makes an important difference to a community whether the inevitably inordinate share of wealth owned by the rich increases substantially or not. For, as Robert J. Lampman has noted, "there is doubtless a maximum degree of concentration of wealth which is tolerable in a democracy" (*The Share of Top Wealth-Holders in National Wealth, 1922-1956* [1962], 6).

Professors McDonald and McWhiney state that my data on wealth are based on an "ahistorical and culture-bound conception of property." After offering a definition of property that raises questions about their grasp of the concept, McDonald and McWhiney conclude that qualitative differences in the forms of wealth in the two sections not only preclude comparison between them but prove that the North and South were "fundamentally different." This last conclusion is, of course, nonsensical. And it would be nonsensical even if property in the two sections were as dissimilar as McDonald and McWhiney claim it was. The fact is, however, that nowhere in their work to date on this theme—at least, in the seven articles that they have written individually, jointly, and, in one instance, in collaboration with Ellen Shapiro McDonald—do they offer evidence establishing that grazing rights were "the most important single property right for the vast majority of [the] white inhabitants" of the South.

Livestock were vitally important, and Professors McDonald and McWhiney deserve credit for reminding us anew of the fact. But was the right to graze them on unfenced land more important than corn, cotton, landownership, or slavery? As for the impossibility of measuring the value of the grazing right, I have a suggestion. Precision is out of the question, true, but equally so for the many Northern animal-owners who Percy W. Bidwell, John I.

Falconer, and Paul W. Gates have shown had a similar right. Census-takers at mid-century asked Southerners as well as Northerners the value of their livestock. Why not do as the enumerators did and accept the partial truths people told them about the value of their animal and other property, without worrying about the insoluble problem of ascertaining the precise value of the pigs' right to eat acorns and nuts in woods not owned by their masters?

Now to politics and power. Professors McDonald and McWhiney argue that the antebellum South's fencing and tax laws demonstrate the lack of power of the planters. Power is, I agree, more sensibly judged by examining the likely consequences of approved policies than by examining the social characteristics of the policy-makers. The problem is not with this general principle but with McDonald and McWhiney's application of it. The few sentences that Lewis C. Gray devoted to the theme in his classic discussion of *Agriculture in the Southern United States to 1860* (1933) do not suggest that grazing rights and fencing laws were very much on the minds of many great propertyowners. One does wish to know whether wealthy Southerners were really dedicated to taxing pigs but forced to give ground on the burning issue. Perhaps, as in the antebellum North, rich and powerful men were prepared occasionally to lose a few. John Jacob Astor, Peter Chardon Brooks, Hezekiah Beers Pierrepont, and men of their sort—great propertyowners all—bore almost all of the (small) tax burden of their communities. Does this demonstrate their powerlessness?

One can only shake his head in disbelief at Professors McDonald and McWhiney's final argument that the very number of Civil War dead proves the fundamental dissimilarity of the North and the South. They appear indifferent to Bell Irwin Wiley's evidence that soldiers in both sections shared common values. And what could be more ironic than McDonald and McWhiney's criticism of using "quantitative data in a way that belies common sense"? For they have devoted massive time and effort to gathering quantitative data on the distribution of what they call Celts within the antebellum American population, an exercise they are engaged in on little more than the bizarre assumption that such evidence will best explain the behavior of the South and North, if not all of American history.

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Baruch College, and
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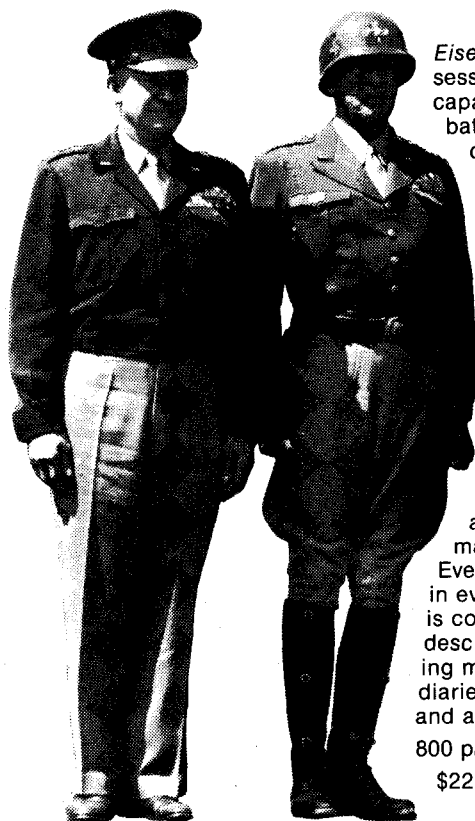
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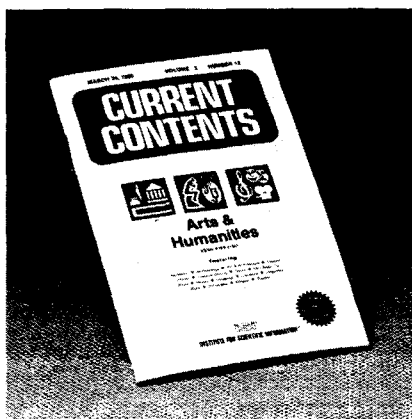
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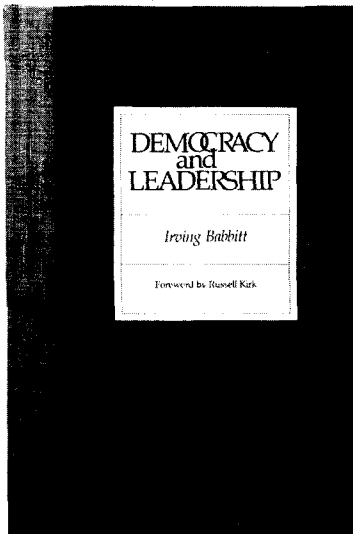
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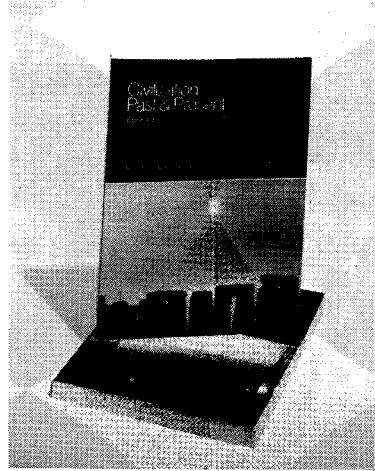
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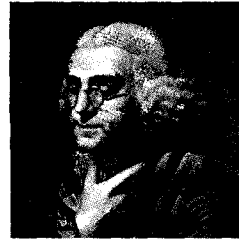
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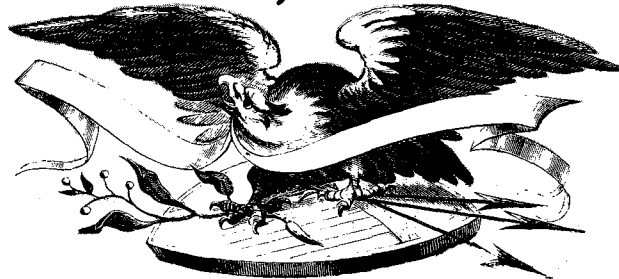
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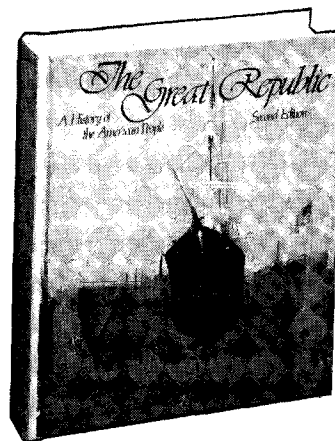
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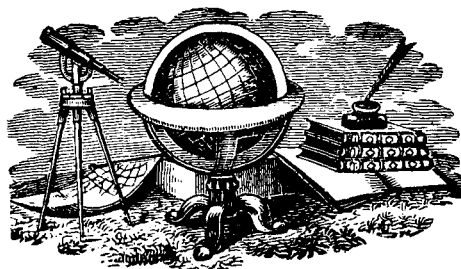
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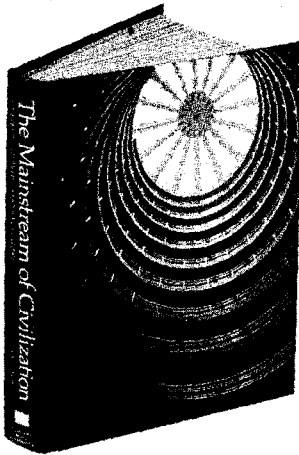
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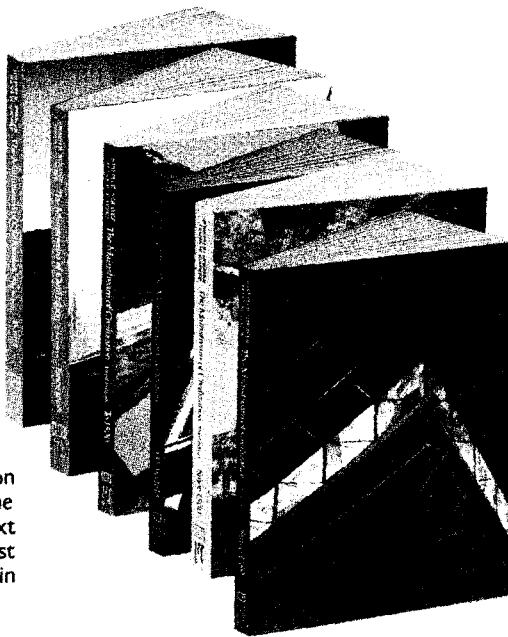
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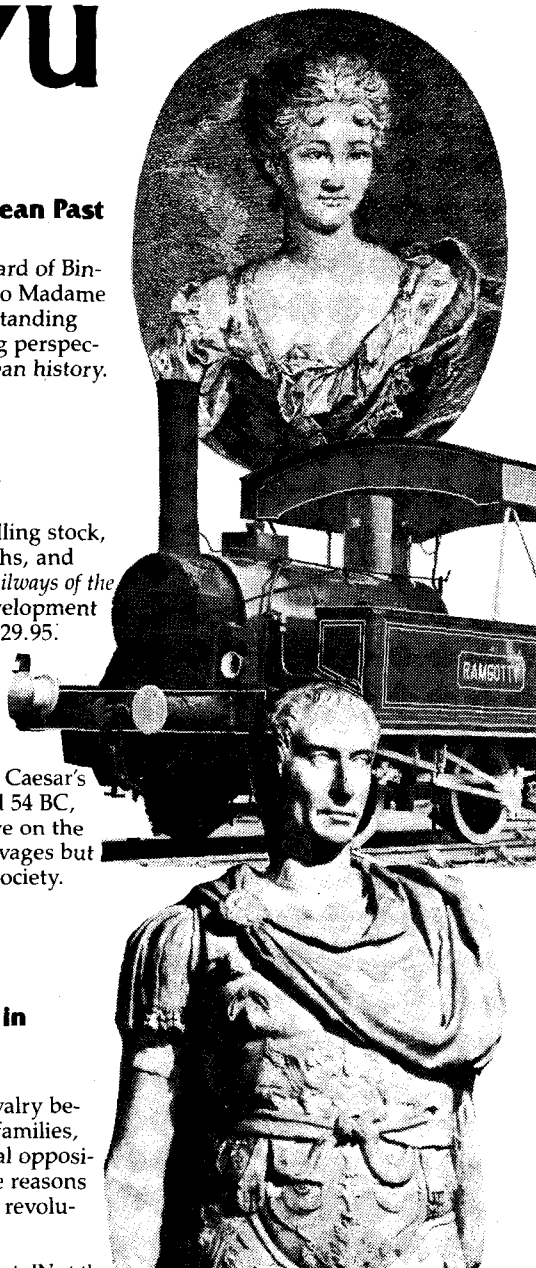
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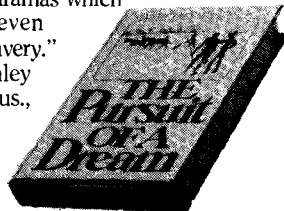


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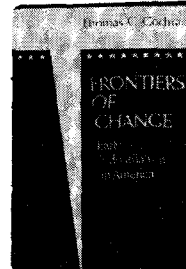
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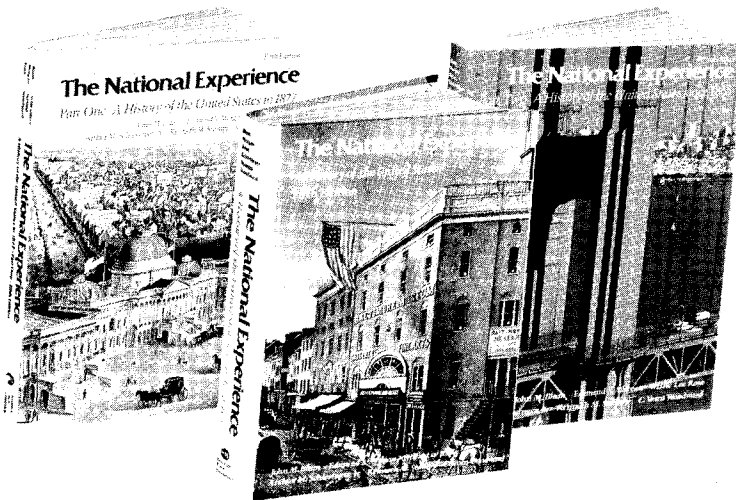
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